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OF THE

Royal Army Medical Corps

EDITED BY

COLONEL SIR WILLIAM H. HORROCKS, K.C.M.G., C.B.

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COLONEL D. HARVEY, C.M.G., C.B.E., R.A.M.C.

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Original Communications.

MOSQUITO NETTING.

By Brevet Lieutenant-Colonel W. P. MACARTHUR, M.D., F.R.C.P.Irel.
Royal Army Medical Corps.

Introductory.

A good deal of confusion exists on the subject of mosquito netting, especially as regards the factors determining the mesh and the methods employed in its calculation. Textbooks are largely to blame for the prevailing lack of acquaintance with a matter of much practical importance, for though most books dealing with tropical hygiene advocate or condemn the employment of mesh of stated counts, I know of none which explains how such counts should be carried out. Apparently the student of hygiene is assumed to be endowed with a degree of knowledge not always vouchsafed to the experienced sanitarian, on the principle, we must suppose, that things hidden from the wise and prudent are revealed unto babes. Indeed the writers of more than one recognized textbook appear to imagine that cotton nets and wire gauze are standardized by the same methods of estimation, and recommendations regarding the mesh of cotton netting required for a certain purpose are frequently based on observations made with wire gauze. How often are we informed that certain mosquitoes can, or cannot, pass through cotton netting of so many holes to the linear inch. Such an assertion is vague to absurdity. At once the questions arise: How was the linear inch measured—along the warp, along the bobbin, or in some other direction? also, What was the grade of cotton used? A mosquito which could pass through a net woven of, say 100/s cotton might easily be kept back by 30/s cotton in a net of the same count per inch.

In my own case, I found the incomplete, and, as I now realize, inaccurate statements concerning mosquito netting so bewildering that I have been able to obtain a comprehension of the factors involved only by going into the subject for myself, and by observing the process of the manufacture of nets from start to finish. I hope that this simple explanatory paper may help to lighten the darkness of others who may have found themselves similarly benighted.

Mosquito Wire Gauze.

Mosquito Wire Gauze, ordinarily known as "Screencloth," consists of two sets of parallel wire strands crossing one another at right angles, and thus forming square openings. The calculation of the mesh is very simple as it is merely necessary to count the number of holes to the linear inch. The inch is measured along one of the two lines of holes and from centre to centre of wires. But obviously the size of the apertures depends not only on the number of holes per inch, but also on the gauge of the wire used. This is calculated in terms of an Imperial Standard which came into operation by Order of Council in March, 1884. In Imperial Standard Wire Gauze a descriptive number indicates the diameter of the wire. Thus "7/0" I.S.W.G. has a diameter of 0·5″. The descriptive numbers then decrease by units to "0," which has a diameter of 0·324″. "1" measures 0·3″, and the numbers then rise by units to "50," which has a diameter of 0·001″. We are mainly concerned with the gauge of wire round about "30," and the measurements of some of these numbers may be given.

Descriptive number I.S.W.G.	Diameter in decimals of an inch
28	0·0148
29	0·0136
30	0·0124
31	0·0116
32	0·0108
33	0·01
34	0·0092
35	0·0084
36	0·0076
37	0·0068

The table on p. 3, based on information kindly supplied by Messrs. Greenings and Sons, of Warrington, correlates the mesh, I.S.W.G., the size of the aperture, and the percentage of daylight admitted, in varieties of screencloth likely to be employed by sanitarians.

The composition of the wire used for screencloth is also a matter of concern. The following materials are in ordinary use. Steel (painted); Galvanized; Brass; Copper; Oxydized Copper; Monel Metal. For dry tropical climates any of these may be used, but for localities where the corroding effects of damp have to be considered, Monel Metal is undoubtedly the best of those mentioned. Messrs. Greenings advocate a special composition of their own, "Grenite," for such climates. As an example of the

difference in price according to the material employed, we may take 14 mesh screencloth, of 32 I.S.W.G. This would cost at to-day's[1] quotations, per 100 square feet:—

	s.	d.
Steel (painted any colour)	25	1
Galvanized	29	1
Brass, copper, and oxydized copper	39	7
Monel metal	62	6
"Grenite"	62	6

TABLE.

Mesh, per inch	I.S.W.G.	Diameter, inch	Aperture	Daylight, per cent.
12	28	0·0148	0·0685	67·5684
12	30	0·0124	0·0709	72·3861
12	32	0·0108	0·0725	75·6900
14	30	0·0124	0·0590	68·2276
14	32	0·0108	0·0606	71·9782
16	30	0·0124	0·0501	64·2563
16	32	0·0108	0·0517	68·4260
17	30	0·0124	0·0464	62·2205
18	30	0·0124	0·0431	60·1866
18	31	0·0116	0·0439	62·4416
18	32	0·0108	0·0447	64·7381
18	33	0·01	0·0455	67·0761
20	31	0·0116	0·0384	58·9824
20	32	0·0108	0·0392	61·4656
20	33	0·01	0·04	64·00
20	34	0·0092	0·0408	66·5856
24	36	0·0076	0·0340	66·5856
28	36	0·0076	0·0281	61·9054

COTTON MOSQUITO NETTING.

This differs in many respects from screencloth. The threads of the warp, and those of the bobbin, or woof, are interwoven so that the mesh consists of two sets of parallel lines of holes intersecting one another at an angle of about 60°. These openings vary in shape from hexagonal to round, and the more nearly circular the holes, the better is the netting.

Unfortunately the mesh of such material cannot be stated accurately in terms of holes to the linear inch, for the count along the warp may differ from that made along the bobbin which are rarely, if ever, the same.

Therefore manufacturers do not recognize this method of estimation and adopt a more reliable one. Indeed, Messrs. Thos. Adams, of Nottingham, tell me that they would refuse to guarantee the mesh of any mosquito netting calculated by a linear inch count. The method adopted is as follows: First of all it is necessary to distinguish the threads of the warp from those of the bobbin.

This can easily be done with the help of a hand lens, and with practice the difference can usually be made out by the unaided eye. The threads

[1] October 23, 1922.

of the warp are more intimately interwoven than those of the bobbin, which have a looser appearance and can often be seen to run free for a distance roughly equivalent to the diameter of a hole. (Fig. 1.) Having differentiated the warp and the bobbin, a count is made of the number of holes along a line of the warp, and a line of the bobbin, falling within a superimposed square inch. Failing anything more elaborate, a piece of paper with a hole an inch square in the centre will be found suitable. The paper is placed so that the lower side of the hollow square inch coincides with a line of holes of the warp, and the paper is manipulated so that a bobbin line of holes meets the first mentioned row at one corner of the square. The mesh is then the sum of the count made along the portions of these two rows falling within the hollow square inch, the hole at the angle of the square, where the two lines meet, being counted twice. Thus, the mesh of the net in fig. 2 is the sum of the counts along the lines AB, AC, and this result is stated as 26 holes to the square inch, or shortly, as 26 hole mesh. Half holes are counted as such; fractions larger than a half are taken as the next higher whole number. The mesh of netting should be correct to within one hole of its nominal count, and this margin of error is often allowed for in contracts by specifying for, say, a mesh of 25/26 holes per square inch.

Just as in the case of screencloth, the size of the openings in cotton netting of the same count will vary with the thickness of the thread used. Obviously the method of standardization according to diameter which is adopted for wire cannot be employed for material like cotton. This is graded according to weight, and in terms of the ratio of the accepted factor of 840 yards to 1 lb. Thus, "30/s" cotton means that such thread goes 30 times 840 yards to 1 lb. The cotton employed for mosquito nets usually varies from 16/s to 100/s, whilst in the finer grades, known to the trade as "Bobbin" netting, the cotton used varies from 80/s to 300/s. Frequently nets are not woven of the same grade of cotton throughout, a thicker thread being used for the warp than for the bobbin. A very common combination for mosquito netting is 40/60, that is 40/s cotton in the warp, and 60/s in the bobbin. Sometimes two cotton threads are twisted and spun as one, which is expressed as 30/2, 60/2, etc. This gives increased strength but costs more to weave. Figs. 3—7 illustrate various meshes in different grades of cotton. Figs 6 and 7 show samples of "sandfly" netting supplied as having the same nominal count. As a matter of fact, counts done with the netting show the mesh to vary from 45 to 48 holes per square inch. But this dissimilarity would be imperceptible to the eye, and the obvious difference in the fineness of the mesh is due to the grade of cotton used, which is the point it is desired to emphasize.

The size of the aperture is also influenced somewhat by the number of "points" on the machine used for weaving the netting. I do not think that this variation is enough to be of any material importance, and it should suffice for practical purposes to pay attention to the actual count, and the grade of cotton employed.

At one stage of manufacture the netting is stretched on special frames and treated with a starch mixture. Exposure to damp, either in washing or as a climatic condition, removes the starch and the loss of its stiffening effect causes the fabric to "fall together" to some extent. Also, the little projecting fibrils of cotton, which always more or less obstruct the fair-way of the mesh, are increased in number; and there is some actual shrinkage of the cotton, so that the count per square inch goes up by about one hole. As a result of these changes the netting becomes less permeable than when it was new, a fact always to be borne in mind. Very finely woven "sandfly" netting might assume with use the density of cotton cloth. In calculating the quantity of netting required for some specific purpose, the shrinkage due to a damp climate and to washing must not be forgotten. A margin of five per cent should be allowed for loss due to these two causes combined.

Mosquito netting does not stretch to any material extent. Strain applied in one direction merely distorts the holes, making them temporarily longer and narrower, and a subsequent compensatory pull rights the mesh again. I have subjected stout mosquito netting to severe strain applied in turn in every direction without making any difference in the subsequent count. Of course, I refer to actual stretching of the fabric, for obviously a count may vary a little according to whether the netting is slack, or pulled taut.

To give an idea of the prices of netting, I have abstracted some recent wholesale quotations. The width of the material differs in the samples quoted for, but this may be reduced to a common factor, if desired, and the cost will fall in proportion to the reduction in the width.

25 hole mesh	..	30/s cotton	..	36 in. wide	..	7¾d. per yard.
,,	..	40/60 ,,	..	108 ,, ,,	..	15½d. ,,
,,	..	80/s ,,	..	36 ,, ,,	..	5½d. ,,
,,	..	100/s ,,	..	36 ,, ,,	..	5½d. ,,
34 ,,	..	80/s ,,	..	100 ,, ,,	..	22½d. ,,
,, ,,	..	100/s ,,	..	72 ,, ,,	..	15¼d. ,,

The foregoing account should indicate sufficiently for our purpose the more important and practical differences between screencloth and cotton netting. The method of calculating the mesh of screencloth is simple and foolproof. That which has to be adopted for cotton netting is unfortunately more difficult, but it can easily be learned with a little effort. I know of no other which could replace it—apart from the inadvisability of having more than one system of counting—for it is necessary to check the number of holes both in the warp and in the bobbin, and to include in the estimation the vertical diameter of the apertures as well as the horizontal. A person who is too careless to learn the recognized system of counting cotton mesh would not likely produce a dependable result by any trouble-saving method that might be devised for his convenience.

THE MESH OF SCREENCLOTH, AND COTTON NETTING, NECESSARY TO EXCLUDE MOSQUITOES.

The experiments to be mentioned later were carried out with *Aëdes (Stegomyia) argenteus* (Poiret), which I believe to be the correct name for the yellow fever mosquito. However as this paper may interest some who do not concern themselves with the niceties of nomenclature, I shall use throughout the more popular name of *Stegomyia fasciata* (Fabricius), which will be familiar to all. Most recommendations regarding the mesh required to keep back mosquitoes are stated in terms of *S. fasciata*, for it appears to be accepted that mesh excluding this species is unlikely to be passed by any other mosquitoes.

Le Prince and Orenstein [1] state regarding screencloth that 16 mesh gauze will permit the passage of *S. fasciata* "only under stress of circumstance," that 17 and 18 mesh will exclude this species, and that 16 mesh will exclude *Anopheles*. The gauge of the wire recommended is 28, 30, and 31 respectively.

I have not succeeded in obtaining an account of the experiments on which these recommendations are based, but I understand that some, at any rate, consisted in placing wire gauze over water containing mosquito larvæ, and noting what mesh the insects passed through on emerging. Experiments of this kind are essential to learn the mesh required for screening tanks, but the action of freshly emerged insects is not necessarily a criterion of their behaviour later when their chitin has hardened and set. A bluebottle just emerged may burrow through several feet of earth, or force itself through an aperture so small that the feat seems impossible until actually seen, but the mature fly would never undertake any such adventures.

The West African Yellow Fever Commission [2] unfortunately did not carry out any set experiments on mosquito netting. This is a pity, for the work would have been done with the painstaking thoroughness which characterized the rest of their labours. They contented themselves by noting the netting required to retain *S. fasciata* for their experiments on its biology—which often concerned freshly emerged specimens—and based their recommendations on these observations. They do not assist us much in deciding what obstruction a mosquito will negotiate under natural conditions in order to obtain blood. Moreover, the mesh of the cotton nets is given in terms of two linear inch counts, presumably measured along the warp and the bobbin, though this is not stated, and there is no reference to the grade of cotton employed. The Commission considered that a mesh of "18 × 18 to the inch" is required to exclude *S. fasciata*, and from the context both screencloth and cotton netting appear to be included. The question of the grade of the wire or thread to be employed is not even mentioned.

My own observations do not accord with this opinion, and I am sure that others have found such a mesh unnecessary. When abroad I have

often been routed by the furious onslaughts of *Stegomyia* during the afternoon, and have retired under a mosquito net where I enjoyed complete immunity from attack, although theoretically the insects should have passed through the mesh with ease. They merely flew up and down outside the net, touching it lightly here and there, occasionally alighting and trying to bite my arm when I moved it close to the net. I have never used a mosquito net with a mesh of anything like 18 × 18 to the inch, (this would mean a count of about 40 by the trade measurement), and I have never had a mosquito come through my net, though I have often spent hours tempting them. Some time ago I went to Nottingham in response to a kind invitation from Messrs. Thos. Adams. Their looms were then busy on large American contracts for mosquito nets of mesh varying from 18 to 24 holes to the square inch, woven of 40/60 cotton. According to the recommendations given above such nets should be useless. The Americans, one imagines, are not so simple as to waste their money on mosquito nets which will not keep out mosquitoes.

Experiments and Tests.

These fall into two groups. In the first I was engaged on breeding *S. fasciata* to obtain all stages for class purposes, the number of adults at any one time varying from approximately 12 to 40. These were kept in cages where they had room to fly about, and where they bred freely. The cages were made of mosquito netting of 24 holes to the square inch, woven of medium cotton. I did not carry out any set experiments, but when the mosquitoes were hungry I used to hold my hand close to the cage in order to tempt them to come through. If my hand was near enough they would try to bite through the netting, but none ever tried to pass the mesh. Subsequently when I inserted my hand into the cage, the mosquitoes would bite freely. I carried out such tests during a period of two months, and did not succeed in inducing any mosquito to force its way through the mesh.

In the second group the tests were more in the nature of set experiments, and some of them are described in detail hereunder. Only small numbers of mosquitoes were employed, first, because I had not many to work with; and further, when one is dealing with a few mosquitoes it is easier to observe their behaviour during an experiment, and also to satisfy oneself that they are hungry and willing to feed both before a test begins and after its completion.

I do not consider that the procedure in which mosquitoes are confined in small tubes, with a piece of netting stretched tightly over the end, can be taken as indicating their behaviour in nature. Under these conditions the insects are frightened and eager to escape, and perforce centre their attack on a tiny piece of netting probably far more drawn out than the most taut mosquito net could ever be. Further, for the free mosquito there is the distraction of a large area of net to be scrutinized, with a

consequent lessening of effort at any one point. For these reasons, cages 14″ × $8\frac{1}{4}$″ × 9″ were employed for the experiments with cotton netting. The cages were divided into two compartments by a transverse partition made of the netting to be tested, and drawn taut. The mosquitoes were put into one compartment and were tempted by various means to pass through to the other. The air in the cages was kept moist by means of sponges, and damp cloths, and the temperature usually ranged from 20—25° C. The willingness of the mosquitoes to feed was always ascertained both before and after an experiment under the conditions which obtained during the test.

Two varieties of netting were tested. One of 25 holes to the square inch, woven of 100/s cotton, the other of 20 holes to the square inch, woven of 40/60 cotton. The cotton in the first is too fine for ordinary use in a mesh of this count, and the second, owing to the large size of the holes in proportion to the grade of cotton, is too easily dragged out of shape to stand hard wear. They were chosen to allow an extra margin of safety as one would recommend for use a finer mesh than that employed in these tests.

The details of some of the experiments may now be given :—

Ex. I, 26 hole mesh, 100/s cotton.—5 ♀ *S. fasciata* were kept unfed for 48 hours. They were then hungry and anxious to feed and were placed in one compartment of the cage. In the other I kept my hand close to the partition for 2 hours. None came through. They attacked my hand at once when given the opportunity.

Ex. IX, same netting.—In a cage as above, 3 ♀ *S. fasciata* were kept for 16 hours with a large square of cotton wool soaked in sugar and water (on which they feed eagerly), hanging on the other side of the partition just beyond their reach. None passed through. When the test was ended, 2 commenced to feed immediately, and the other a little later.

Ex. XIV, same netting.—2 ♀ *S. fasciata* were placed in one compartment of the cage with ample food close to the partition, as in *Ex. IX*. Neither passed through the netting but remained on their own side of the partition where they starved to death. One died on the 9th day of the experiment, the other on the 10th.

Ex. X, 20 hole mesh, 40/60 cotton.—5 ♀ *S. fasciata,* 4 of which attempted to bite my hand before the test commenced, were placed in one side of a cage, as above. I kept my hand in the other side close to the partition for 16 hours (3 hours; 1 hour's interval; and 13 hours continuous). None passed through. When the test ended at 6 a.m., 3 of the mosquitoes commenced to feed on my hand immediately, and the other 2 a few minutes later. In this and the following experiment, the mosquitoes were kept in the dark from about 10 p.m., except that occasionally I switched on the light for a few seconds to read the thermometer in the cage, and to make certain that the degree of humidity was correct.

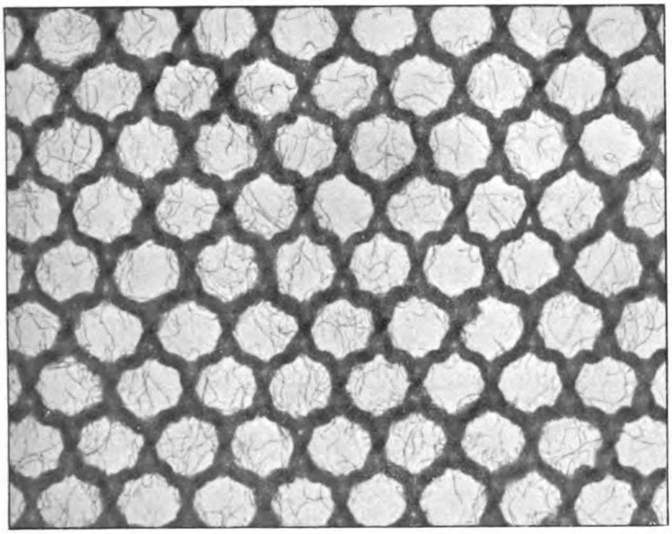

FIG. 1.—Mosquito netting enlarged to show the different appearance of the warp (horizontal), and the bobbin, or woof (diagonal), which must be distinguished in counting mesh. Some of the fine cotton fibrils can be seen; these would increase in number with wear and washing. All the netting used for the purposes of this paper was new and unwashed.

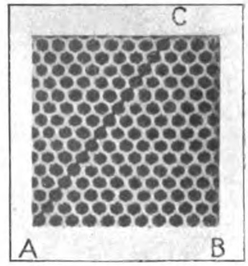

FIG. 2.—Shows the correct method of counting the mesh of cotton netting. The mesh of this net is the sum of the counts made along the lines A B and A C, the hole at A being counted twice.

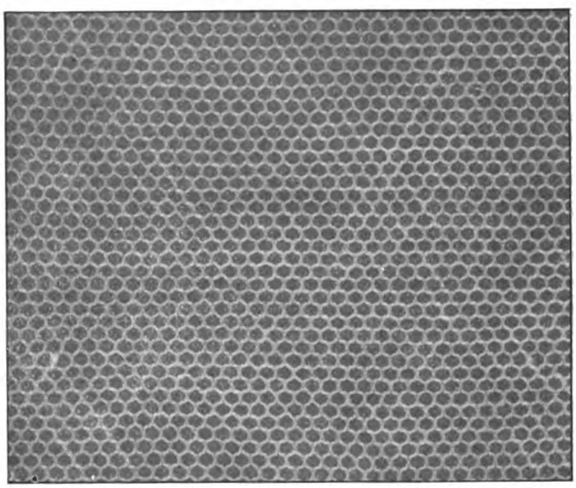

FIG. 3.—Mosquito netting, 25/26 holes to the square inch, 30/s cotton.

To illustrate "Mosquito Netting," by Brevet Lieutenant-Colonel W. P. MACARTHUR, M.D., F.R.C.P.Irel., R.A.M.C.

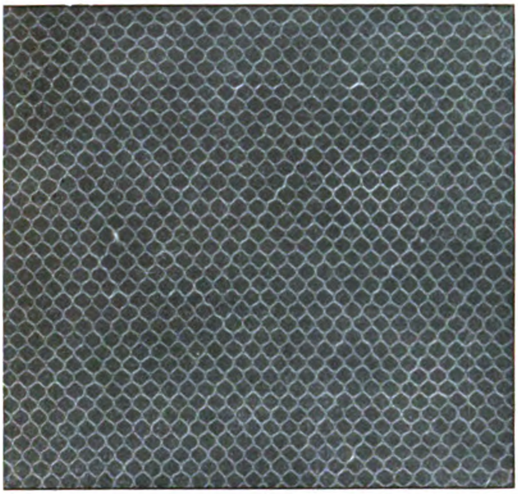

FIG. 4.—Mosquito netting. Same nominal count as Fig. 3., 80/s cotton.

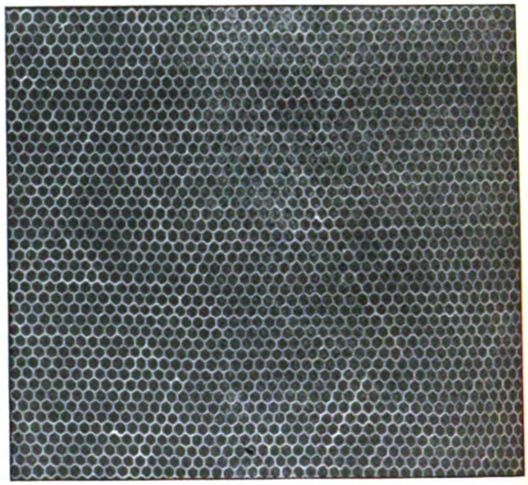

Fig. 5.—Mosquito netting, 34/35 holes to the square inch, 100/s cotton. This mesh cannot be woven in cotton coarser than 80/s.

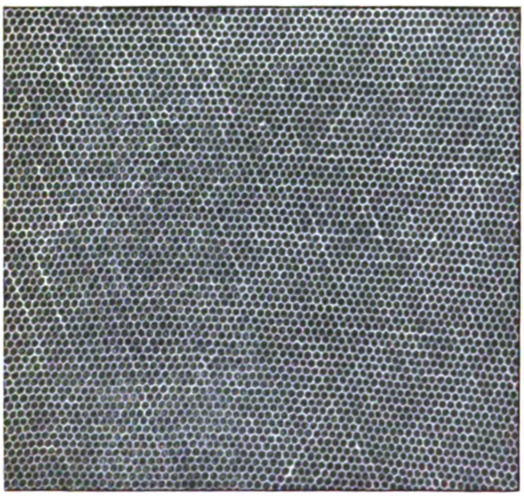

FIG. 6—"Sandfly," (*Phlebotomus*), netting. 80/100 cotton. This mesh cannot be woven in any coarser cotton.

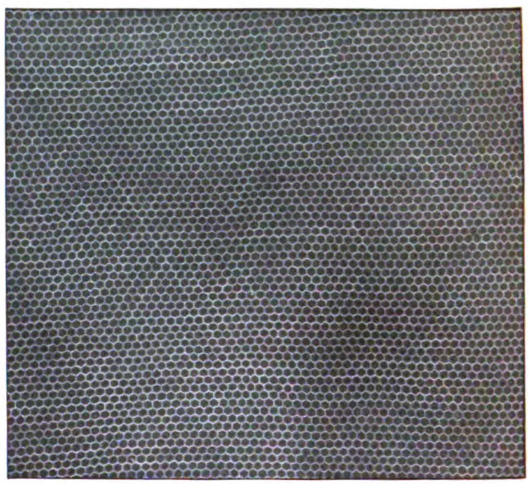

FIG. 7. — "Sandfly" netting, same nominal count as Fig. 6 (see text). 180/s cotton.

To illustrate "Mosquito Netting," by Brevet Lieutenant-Colonel W. P. MACARTHUR, M.D., F.R.C.P.Irel., R.A.M.C.

Ex. XI, same netting.—4 ♀ *S. fasciata* were unfed for 24 hours, and caged as usual. I kept my hand in the other side of the cage, as above, for 18 hours (3 hours; 1 hour's interval; and 15 hours continuous). When the test ended at 7 a.m. my hand was attacked by 3 mosquitoes, the 4th did not bite. Later it laid several eggs, which may explain its failure to feed.

The mosquitoes not employed in an experiment were kept in a cage made of the netting used in the two tests just described. They were counted night and morning and none escaped. The Curator of Insects at the Zoological Gardens tells me that she has used this mesh for *S. fasciata* cages for two years continuously, and has found it quite satisfactory. She knows of only one escape during this time, a small male which passed through the netting in a successful attempt to join its friends.

The experiments with screencloth were carried out on the general lines of those already described, except that the same cages could not be used as the pieces of screencloth to be tested were too small. Cages 4½″ × 4½″ × 4″ were prepared; these were not small enough either to frighten the mosquitoes or to cramp them. One side of the cage consisted of the gauze to be tested, the other sides being impassable. The small cage was placed inside a large one, and the mosquitoes were tempted to pass through the wire gauze by the means already described. After several negative experiments with 12 mesh screencloth of 28 I.S.W.G. a positive result was obtained. In this instance, 5 *S. fasciata* were confined as usual, and on the third day of the test one succeeded in escaping through the wire gauze.

Similar tests repeated with 14 mesh screencloth of 30 I.S.W.G. gave uniformly negative results.

I thought I might save myself sleepless nights by using a black guinea-pig as a bait instead of my hand, but the guinea-pig proved less attractive to the mosquitoes than a mixture of sugar and water.

Other tests on the lines of those described were carried out, and various additional means were tried to tempt the mosquitoes to come through the netting. Thus, the mosquitoes' end of the cage was exposed to the sun so as to cause them obvious discomfort, the other compartment being kept cool and dark in order to appear attractive. Also, the sexes were separated and used as baits for one another. In one test a male, included with the females for company, succeeded in getting through the 20 hole netting, but I attach no importance to this, for, of course, males are smaller than females, and as they do not suck blood there is no inducement for them to pass through mosquito nets under natural conditions. With the exception of this one male, no mosquitoes got through the two varieties of cotton netting tested.

One female (in one test out of many), passed the 12 mesh screencloth, and none of either sex got through the screencloth of 14 mesh. I consider that there is a stronger inducement in these experiments for *S. fasciata* to attempt to force its way through netting than any that is likely to obtain

in nature, where an alternative food supply in the form of human beings, dogs, goats, rats, etc., would probably be available.

The mosquitoes used for these experiments were laboratory bred, and were of the usual size. I compared pinned individuals with similar specimens of *S. fasciata* caught wild in Malta, Africa, Mauritius, etc., and could distinguish no difference in bulk. In laboratory experiments one hears a great deal about dwarf specimens of mosquitoes bred out by starving the larvæ. It would be very interesting to know to what extent these occur amongst wild mosquitoes.

Conclusions, etc.

I believe that cotton netting of 25/26 holes to the square inch, and woven of 30/s, or 40/60, cotton, will exclude mosquitoes when used as a mosquito curtain under natural conditions.

As regards screencloth, I think that screencloth of 14 mesh to the inch and 30 I.S.W.G. should suffice to exclude mosquitoes from buildings. As is well known, the eggs of *S. fasciata* can withstand drying for long periods and they may subsequently be washed into tanks and other containers when rain comes. Such receptacles should be screened with 18 mesh gauze, as freshly emerged *S. fasciata* might struggle through a coarser mesh before their exo-skeleton sets. Mosquitoes at this stage do not suck blood.

If buildings are screened with unnecessarily fine screencloth no harm is done, for this material is more or less of a fixture and cannot be discarded at the will of the individual. Whereas there is grave objection to the use of cotton netting any finer than is absolutely necessary, as will be realized by medical officers who have experienced the difficulty of inducing troops to remain under their nets at night, especially in a hot, steamy climate. The finer the mesh, other things being equal, the greater is the temptation to discard the nets.

There is need for further observations on the mesh necessary to exclude mosquitoes and "sandflies," respectively, not in laboratory tests but in actual practice. We are not concerned with what mesh a frenzied mosquito imprisoned in a test tube may struggle through in order to escape from captivity, but with the mesh that a free mosquito will pass under natural conditions to obtain food. Any medical officer with a working knowledge of netting could carry out most instructive experiments in infested districts. Owing to the many possible sources of error, the tests would have to be very carefully carried out, and in order to attribute any importance to the presence of mosquitoes inside a net it would be necessary for the observer to have seen them himself actually coming through the mesh. If it is true that a mesh of about 40 to the square inch is necessary to exclude *S. fasciata*, his vigil under a net of 25 hole mesh should not be a very lengthy affair.

ACKNOWLEDGEMENTS.

I wish to thank Messrs. Greenings and Sons, of Warrington, and Messrs. Thos. Adams, Ltd., of Nottingham, for supplying the screencloth and cotton netting, respectively, used in these experiments, and for many other kindnesses received. The non-arrival of promised *S. fasciata* eggs delayed the tests, and I am much obliged to Miss Cheeseman, Curator of Insects to the Zoological Society of London, for providing sufficient adults to complete the work intended.

REFERENCES.

[1] "Mosquito Control in Panama," p. 206.
[2] Yellow Fever Commission (West Africa) Report III, p. 167.

SEROLOGICAL EXAMINATION OF ONE HUNDRED STRAINS OF THE GONOCOCCUS ISOLATED FROM CASES OF ACUTE AND SUBACUTE URETHRITIS IN THE MALE.[1]

A Report to the Medical Research Council.

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I.—Object of the Investigation.

The immediate objects of the investigation herein described are two-fold:—

(*a*) In the first place it is necessary that the Gonococcus be surveyed from the standpoint of its serological characters in order that a serious attempt be made to determine the value of vaccine therapy in gonorrhœa.

Unless such a survey be made, the employment of stock vaccines for therapeutical purposes, even although the vaccines be prepared from several strains of the micro-organism, is always open to the obvious criticism that the serological characters of the strains used for preparing the vaccines cannot be correlated with those of the organism responsible for the causation of the disease in any case, or in any group of cases, of gonorrhœa. If, however, it be shown that a large percentage of cases of gonorrhœa is due to one "type," or even to closely related "types" of the gonococcus, it should be possible to make an adequate statistical examination of the value of vaccine therapeutics in this disease.

So far, work on this problem has proceeded in two directions:—

(i) On the one hand, it has been suggested that a vaccine be prepared for each individual case: this ideal unfortunately presents difficulties which at present are almost insurmountable, for its application in venereal diseases clinics throughout the country presupposes that the necessary technique is simple and presents but little difficulty to the average bacteriologist. In the present state of our knowledge this is far from being the case.

(ii) On the other hand, polyvalent vaccines have been used extensively, but in preparing these it has been assumed, mainly as a result of the work of Torrey (1907-1908) [18], that the antigenic qualities of the gonococcus vary so markedly that each strain must, from the viewpoint of immunology, be considered, potentially at least, as individual. To prepare a vaccine for routine use it has, therefore, been considered neces-

[1] With acknowledgements to the Editors of the *Journal of Pathology and Bacteriology*.

sary that a relatively large number of strains be represented in the therapeutic inoculum, and latterly this has become so extreme that a state of things suspiciously akin to polypharmacy has developed. If a vaccine contain twelve to fifteen different strains of the gonococcus, and presumably only one of these corresponds serologically to the micro-organism responsible for the infection, then a very much larger dose of the stock vaccine than that which would be employed in the case of an autogenous vaccine, would—*à priori*—be needed to obtain the same immunizing effect. Owing to the toxicity of suspensions of the gonococcus, these large doses cannot be given and in consequence methods have been designed for making the vaccine less toxic. These, however, present inherent disadvantages in that split products of the organism—which products are certainly toxic—obtained by digestion with ferments or alkalies are discarded, and it may well be that the discarded material is not without some immunological significance. If any method of detoxication is to be employed the process of detoxication should be such that *all* the products of the organismal protoplasm are incorporated in the vaccine, for Douglas (1921) [8] has shown that while the inoculation of certain split products of bacteria into animals leads to the production of bactericidal immune bodies, animals so immunized lack both those immune bodies which produce agglutination and those which stimulate phagocytosis.

There is one method of detoxicating a vaccine which is not open to this criticism—sensitization by the method of Besredka (1902) [2]. If, however, sensitized vaccine were to be used generally in venereal diseases clinics it would be necessary to know the relative importance and the distribution of the serological types of gonococcus to be sensitized so that appropriate sera be prepared for their sensitization.

(*b*) The second, and I feel the more important, object of the inquiry is to prepare the way for elaborating a method for satisfactorily conducting complement fixation or other serological tests for the diagnosis of chronic gonorrhœa, especially in the female.

The need for such a test is clamant, for chronic gonorrhœa of the female constitutes the reservoir of infection for a not inconsiderable proportion of cases of the disease, and is moreover an important, and in the opinion of some authorities the *most* important, cause of sterility in the female.

Chronic gonorrhœa of the male also presents some difficulty in diagnosis and is also not infrequently responsible for the causation of sterility, so that in this connexion, too, a satisfactory serological method of diagnosis should prove valuable.

Until, however, an immunological survey has been made of a considerable number of gonococci, the value of such tests must remain not only doubtful but the results obtained would be open to serious criticism.

In this connexion also attempts have been made to overcome the difficulty arising from the presumed multiplicity of types of the gonococcus

by employing for routine tests compound antigens comprising numerous strains of the micro-organism.

Now, if complement-fixation reactions be reviewed critically it is apparent that many bacterial antigens employed in such tests are themselves markedly anti-complementary; if, then, we use a compound antigen in such tests, the antigen being composed of numerous strains presumed to be antigenically different, it may be that only one strain represented in the compound antigen forms, with the serum to be tested, a true complement deviating complex, while the other strains, although taking no true part in the deviation, exert an anti-complementary action which may interfere with the specificity of the test. From experiments which have recently been carried out in this laboratory evidence has been forthcoming that this criticism is by no means invalid. These experiments will form the subject of a later communication.

The whole subject is rendered still more difficult when the part played by heterogenetic antibodies in complement deviation is properly appreciated, and it is especially worthy of note that Warden, quoted by Kolmer (1917) [12], has already suggested that the lipoids of the gonococcus freed from the protein of the organism might subserve as the antigen in complement fixation tests in gonorrhœa.

Should such lipoid suspensions bring about complement fixation with gonorrhœal sera, the test becomes comparable to the Wassermann reaction —a purely empirical phenomenon susceptible of examination only by very extended application of the method of trial and error. Moreover, were this so the application of the test would present its own peculiar difficulties in that suspensions of such lipoids might possibly react in presence of certain syphilitic sera to produce pseudo-complexes akin to those formed in the Wassermann test.

This aspect of the subject will be more fully considered in a future communication, for, owing to the frequency with which syphilis and gonorrhœa are encountered in the same case, it is advisable, although perhaps not absolutely essential, that, if possible, only such antigens all the constituents of which are *proved* to be devoid of complement fixing qualities in presence of syphilitic sera, be employed for fixation tests in gonorrhœa.

II.—Culture Methods Employed.

(A) Tests of Media with a view to Determining Optimum Method.

Owing to the fact that the present investigations had to be carried out when the conditions of work in this laboratory were difficult, accommodation being limited, considerable modification of culture methods already described by various authors was made in order to obtain a medium giving abundant growth of the gonococcus. As a preliminary to the elaboration of a method which fulfilled my requirements, tentative trials were made of the following methods:—

(*a*) Cole's medium (1917) [6].
(*b*) Trypagar (1917) [4].
(*c*) Thomson's medium (1917) [17].

None of these media proved in my hands adequate for the purpose in view, as the growths obtained were not sufficiently luxuriant. Twelve tests were made of each of the three media, the material for primary inoculation being derived only from acute cases of urethritis in the male of less than one week's duration, and showing many gonococci on microscopical examination of the urethral exudate.

(1) Cole's medium gave two positive results out of twelve attempts.
(2) Trypagar gave four positive results out of twelve attempts.
(3) Thomson's medium gave nine positive results out of twelve attempts.

These results could not be regarded as wholly satisfactory, but attention is especially called to the fact that in presenting these figures it is not the intention of the author adversely to criticize the methods essayed in these preliminary tests. It is significant that using trypagar, with the preparation of which the staff of this laboratory is familiar, better results were obtained than when Cole's medium was employed, and I therefore think it not improbable that had the use of any one of the above media been persisted in, satisfactory results would ultimately have been achieved.

A series of experiments was then carried out to determine the value of those constituents of the media which were not susceptible to heat, and the fact already noted by Cole that amino-acids are especially valuable as nutrients for the gonococcus was fully corroborated.

(*B*) *Media Finally Elaborated.*

These preliminary experiments made clear that two qualities were essential in any medium for the successful cultivation of the gonococcus:—

(1) The salts and nutrients of the medium which are thermostable.
(2) The special growth-stimulating and sustaining factors which it must contain and which are to a greater or less extent thermolabile.

Bearing these facts in mind, a medium was prepared thus:—

(1) A mixture of:—

Trypsinised broth (Douglas)	500 c.c.
Trypamine (Cole)	100 ,,
KCl	0·38 grm.
$CaCl_2$	0·44 ,,
$NaHCO_3$	0·18 ,,
Crystalline di-sodium hydrogen phosphate	5·00 grms.

is steamed for one hour to promote solution of the reagents and to drive off toluene which is used as a preservative in the preparation and storage of trypamine.

(2) Take twenty-five grammes agar fibre, cut into sections about quarter-inch long and soften by the addition of 0·25 per cent acetic acid which is left in contact with the agar for fifteen minutes. Wash the agar to

remove acid and finally squeeze through lint to get rid of as much water as possible.

(3) Mixture (1) is now transferred to the inner portion of an enamelled double saucepan, the outer water jacket of which should be boiling. The agar (2) is added, and the material is heated thus for twenty to thirty minutes. The contents of the inner pan are now transferred to a flask of 1,000 cubic centimetres capacity and boiled on an open flame for fifteen minutes to complete the solution of the agar. At this stage there is some danger of the thick agar solution "catching" unless the flask be kept moving, especially for the first five minutes, during which boiling on the open flame is proceeding. The reason why the material is here transferred to a flask is that owing to the container being transparent one can readily see when the solution of the agar is complete.

The mixture is brought to a reaction of approximately Ph. 7·6 and is cooled to and maintained at a temperature of 56° C. in an oven or water bath.

(4) Both Cole and Thomson employ extracts of tissue to assist in obtaining luxuriant growths of the gonococcus, and the value of this the author fully corroborates. To extract tissue, take 500 grammes of *fresh* ox-heart, add 500 cubic centimetres of water and immerse in a 56° C. water bath, shaking the container frequently or stirring to equalize the temperature. When the extract has been thus raised to a temperature of 39° to 42° C., put the container in a water bath or incubator at 37° to 42° C., and allow extraction to proceed for two hours.

The extract is finally strained through butter muslin.

(When making the medium this is the first procedure which should be carried out, in order that time may be saved.)

(5) The tissue extract is now added to the concentrated agar nutrient (3), 100 cubic centimetres being added at one time and five minutes being allowed to elapse between each addition of 100 cubic centimetres; the extract is added thus to prevent the colder fluid causing the agar to set, and during the procedure the agar is maintained at 56° C.

(6) When all the tissue extract has been added, the mixture is transferred to the double saucepan, the water jacket of which should be boiling before the transfer is made. This "agar extract mixture" is heated in the double saucepan for twenty minutes in order to coagulate the albumens. The mixture is again standardized to Ph. 7·6 approximately and is cooled to 56° C., when the whipped white of two eggs is added and well mixed.

The whole is heated in the double saucepan for twenty minutes, and here again the outer jacket of the saucepan should be boiling before the inner section containing the mixture is put into it.

(7) The material is now passed through butter muslin and filtered through English Chardin paper (Baird and Tatlock, 14, Cross Street, Hatton Garden, London). Filtration is carried out in an oven at 55° C. and should not take longer than thirty minutes for 1,000 cubic centimetres.

(8) The product is finally adjusted to a reaction of Ph. 7·6 and is transferred for storage to milk bottles of 200 cubic centimetres capacity, each of which contains ten to fifteen cubic centimetres of pea extract.

(9) Sterilization and distribution of medium.—The medium must not be overheated, especially after the addition of heart extract, and if all glassware, butter muslin, paper, etc., be steamed before use there is no need to sterilize the completed medium for more than fifteen minutes on one occasion, *but the steam sterilizer should be up to 100° C. before the bottles or tubes of medium are put into it.*

If a bottle of the medium is to be tubed or plated arrangements must be made to deal with the whole of the contents of the bottle at one time, and all glassware employed must be sterilized before use, as continued or frequent heating quickly reduces and ultimately destroys the growth-stimulating and growth-sustaining properties of the medium.

Note of method used for standardizing the medium.—As Swartz (1920) [22] has shown that provided the other factors of the medium are satisfactory there is no need for great exactitude in regard to its hydrogen ion concentration, the following simple method of standardization has been adopted in this laboratory and is now used as a routine in the medium room.

Materials required:—

(i) White porcelain water-colour palette, with at least six depressions sufficiently deep to contain 1·5 to 2 cubic centimetres.

(ii) One 0·1 cubic centimetre pipette.

(iii) Three 1 cubic centimetre pipettes.

(iv) 0·002 per cent solution of cresol red (Cooper Laboratory, Watford).

(v) Standard solutions of Ph. 7·5 and Ph. 7·7.

(vi) Strong solution of NaOH.

Proceed as follows:—

(*a*) Run into each depression of the palette 0·1 cubic centimetre of cresol red solution.

(*b*) Into upper left-hand depression run 1 cubic centimetre of Ph. 7·5 solution, and into upper right-hand depression a similar quantity of Ph. 7·7 solution. The solution in one case will have the colour of tawny port and in the other will be purple-red.

(*c*) Add 1 cubic centimetre of medium to any of the remaining four depressions and if the colour obtained be midway between that obtained with the two Ph. solutions, the process is completed.

Usually the medium is too acid, and one drops strong caustic soda solution, a few drops at a time, into the bulk of the medium, testing after each addition in the manner above described until the desired tint is obtained.

After a little practice, one soon attains a degree of proficiency which permits of the comparative tinting being discarded until the final adjustment of the reaction. Herein lies the advantage of cresol red as an

indicator for it shows a distinct change from yellow to brown at about Ph. 7·5.

It may be noted that owing to its high buffer content the medium is very easily standardized.

(*C*) *Reason for Detailed Description being given of the Medium Employed.*

The method of preparing the medium has been given in detail for two reasons.

In the first place it has proved to be very consistent and gives luxuriant growths, and secondly, the cultures obtained exhibit antigenic qualities that appear to be fairly constant.

Stress is laid on the latter property as it is well known that alteration in cultural conditions leads not infrequently to variation of the antigenic properties of organisms. Bordet demonstrated this variation in the case of *B. pertussis* in (1909) [3], and since then further evidence of such instability in culture has been forthcoming.

Moreover, unless a medium giving luxuriant growth be employed, the so-called d'Herelle phenomenon (1917, 1918, 1919) [7] is liable to occur in cultures of the organism under consideration. Its occurrence might well lead to the employment of resistent colonies only in subsequent work, and this may vitiate the results of serological examination, for Arkwright (1921) [1] has shown that variations of *Bacillus shiga* which are probably akin to the variations which arise from the d'Herelle phenomenon and which, in the case of *B. coli*, have been fully described by Bordet and Ciuca (1920) [4], exhibit immunological reactions which differ considerably from what might be termed "normal" Shiga bacilli. Hermanies (1921) [10] calls attention to the occurrence of changes in gonococcus cultures which in view of Bordet's work must be interpreted as the d'Herelle phenomenon. The appearances noted by Hermanies were frequently observed in the series of cocci herein considered.

Note on Preparation of Trypamine.—In order that the account of the method of preparing the medium may be as complete as possible, attention is here called to the fact that not all commercial preparations of casein are suitable for making Cole's "trypamine."

My first attempts, in which Laitproto No. 6, as originally advised by Cole, was used, gave unsatisfactory digests. I am indebted to Mr. Cole for a personal communication in which he advised the use of "Protene," another brand of casein, but, unfortunately, this, although it proved valuable, could not be regarded as wholly satisfactory.

It was, therefore, decided to prepare the protein basis of the digest in the laboratory—ox blood is defibrinated and the fibrin is washed in running water overnight. The washed fibrin is well squeezed, is laid out thinly on trays and is dried in the 56° C. oven. When completely dry it is powdered

in a mortar and, as a dry powder, can be stored indefinitely without deterioration.

The pancreatic enzyme is also prepared in the laboratory—obtain the pancreas from five or six pigs, cut away fat, mince, spread thinly on a glass slab and expose to air at room temperature for sixteen hours to activate the trypsinogen. Scrape up the paste of pancreas and put into a mortar, add 500 cubic centimetres absolute alcohol and pound to mix well. Strain through cheese cloth and squeeze. Wash again with 300 cubic centimetres of alcohol and strain. Add to the paste so obtained 200 to 300 cubic centimetres ether, mix well, pounding with a pestle, squeeze through cheese cloth to obtain as dry a product as possible, and finally spread on trays for desiccation in 37° C. incubator.

The method is somewhat wasteful, both in alcohol and enzyme, but the stability of the product and the ease with which it can be handled will repay the initial loss.

Dry pancreas prepared in this way maintains its activity for years; a specimen thus prepared in 1914 was kindly placed at my disposal in February, 1921, by Mr. W. Milne, Steward of the Physiology Department, University College, Dundee, and its activity was but little less than that immediately after its preparation. I wish to place on record my indebtedness to Mr. Milne for calling to my notice this simple and efficient method of preparing dried pancreas.

To prepare trypamine from these reagents :—

Take 500 cubic centimetres of 0·5 per cent solution of (anhydrous) $NaHCO_3$, add fifty grammes dried ox fibrin and heat to 38° C. approximately for two to three hours to cause the fibrin to swell, add five grammes dried pancreas and five cubic centimetres of toluol as a preservative; incubate for forty-eight hours at 37° C. The reaction of the digest is then tested, and by adding a strong solution of NaOH the reaction is adjusted to a point frankly alkaline to cresol red—colour of permanganate solution—but still acid to phenolphthalein. A further 2·5 grammes pancreas is added and incubation is continued for four days.

The digest so prepared is stored with toluol present as a preservative and is added, without further treatment, to the medium in the quantity and under the conditions already mentioned.

Since using this method of preparing trypamine uniformly satisfactory digests have been obtained.

(*D*) *Method of Employing the Medium.*

In isolating the first series of fifty gonococci the medium described in section II (*B*) was used, as follows :—

(i) *For Primary Cultures.*—Tubes containing eight cubic centimetres of medium and of size six inches by three-quarters of an inch are boiled for five minutes to melt the agar aud are then transferred to a water bath at

56° C. When the melted agar has cooled to this temperature (about ten minutes is required) one cubic centimetre of extracted human plasma, prepared as described by Thomson (1917) [17] is added to each tube, the tube sloped and incubated overnight to ensure sterility. If the "human plasma agar" is not to be used within forty-eight hours of its preparation, the tube should be stored in the ice chest.

(ii) *For First Subcultures.*—The agar is melted as for making "human plasma agar," but to each tube of melted medium cooled to 56° C. there is added one cubic centimetre of one in ten dilution of rabbit plasma in place of one cubic centimetre of undiluted human plasma.

The rabbit plasma is obtained by bleeding out, under ether anæsthesia, a rabbit from the carotid into eight cubic centimetres of sterile two per cent neutral citrate, all precautions being taken at the operation to ensure sterility. The plasma is allowed to separate by standing or is separated by centrifugalization; it is then diluted one in ten with sterile 0·85 per cent NaCl. The diluted plasma is transferred to vaccine bottles of twenty cubic centimetres capacity, sterile rubber cups are applied and wired on so that the bottles may be dropped into the 56° C. water bath for thirty minutes. Thereafter, the plasma is stored in a cool place and is used as required—rigid precautions to ensure sterility being, of course, adopted in distributing this reagent.

Before use, the tubes of "rabbit plasma agar" are also incubated overnight at 37° C. to ensure sterility.

(iii) *For Subsequent Subcultures.*—For later subcultures, the medium described may be used without the addition of either human or rabbit plasma, but all gonococci do not grow well on the unenriched medium, and the growths are not so abundant as when rabbit plasma is used.

(iv) *For Stock Cultures.*—The medium is put up in six-inch by half-inch tubes in a quantity of 2·5 cubic centimetres per tube; these are melted, cooled to 56° C., and to each tube is added 1·5 cubic centimetre of one in ten dilution of citrated human plasma. The agar thus diluted sets to a soft jelly, and after being incubated to ensure sterility, the plugs of the tubes are paraffined to maintain the humidity of the medium.

These tubes are used for making stab cultures, and on this medium most strains of the gonococcus remain viable in the 37° C. incubator for six weeks to two months. Some strains are exceptionally viable, one of my strains being readily subcultured after six months, but others are less hardy, and it is advisable to subculture every three weeks, and in the case of recently isolated strains, after ten days if it be desired to ensure the viability of the culture.

(v) *Alternative Method for Maintaining Stock Cultures.*—Inspissated whole egg as described by Gordon for stock cultures of the meningococcus may be used in place of the method described above—(iii).

In examining the second series of fifty strains, a modified technique was used in making the medium for primary cultures.

It is not always easy to obtain sterile human plasma at such time and in such quantity as may be required for the method already described, and on the departure of Mr. D. M. Greig from Dundee to assume the Curatorship of the Royal College of Surgeons Museum in Edinburgh some difficulty was experienced in obtaining this reagent with the regularity necessary for the prosecution of the work. Another objection to the use of human plasma is that if it be not stored at or below 0° C. its growth-stimulating properties deteriorate fairly rapidly, so that unless an efficient cold storage is available the wastage of this reagent is considerable.

I was, therefore, compelled to become myself the source of the human plasma, and for a period of four months and a half was bled to the extent of forty to sixty cubic centimetres at weekly intervals. While no definite ill effects were noted this source of the reagent had to be relinquished owing to slight intercurrent illness, convalescence from which was however unduly protracted.

For these reasons it was decided to attempt to modify the medium in such a way that it would be unnecessary to employ large quantities of natural human protein for primary culture. That the quantity of natural human protein which had been used was in excess of requirement appeared probable from the work of Blair M. Martin (1911) [13], while recent investigations of the growth requirements of bacteria by Cole and Lloyd (1916-17) [6], and by Thjötta and Avery (1921) [16] and others, indicate that the presence of only quite small quantities of the hypothetical growth-stimulating factors resident in natural protein may be required for obtaining luxuriant cultures.

(vi) *Modified Medium for Primary Culture.*—Tubes of the medium described in section II (*B*) are melted and cooled to 56° C., and to each is added one cubic centimetre of one in ten dilution of rabbit plasma and one drop of human blood. The blood is obtained by pricking the thumb which is previously sterilized by rubbing with alcohol, a drop of which is burned off the skin just before the puncture is made. This gives a slightly opaque medium, but if that be found unsatisfactory the difficulty is readily overcome by mixing the blood with five times its volume of sterile water and adding five drops of the blood thus laked to each tube of medium.

The blood may be spread on the surface of the agar, but I find that the procedure is less satisfactory than incorporating it in the medium in the manner just described.

The technique has the great advantage that all the reagents employed can be stored indefinitely in a cool place, or are immediately obtainable in the quantities required.

Pleuritic, ascitic, or hydrocele fluid can be used instead of one in ten rabbit plasma, but these exudates are less definite in constitution than is diluted plasma, and as rabbit plasma is required in any case for making

cultures that are destined for immunological work, no advantage is gained by using these exudates.

This method has proved quite as satisfactory as that originally employed; thus with the original method fifty-eight attempts gave fifty positive results in primary cultures, while with the modified medium fifty positive results were obtained in fifty-four attempts.

(vii) *Special Precaution to be observed in Making Cultures.*—In making both primary cultures and subcultures it is essential that the medium be warmed to 37° C. before inoculation, and thereafter maintained at that temperature. Failure to observe this simple rule will assuredly lead to unsatisfactory results being obtained.

(viii) *Use of Commercial Peptone in Place of Trypamine.*—As the preparation of the medium described is rather troublesome, an attempt was made to substitute a commercial peptone for the mixture of trypsinized ox-heart and "trypamine" used in the original medium herein described.

While growth could be obtained with media prepared thus, the cultures were not so luxuriant as those inoculated on the original medium. One brand of peptone, however, proved very satisfactory indeed, viz., Bacto-difco peptone, prepared by the Digestive Ferments Company of Detroit, Michigan, U.S.A. I am indebted to Dr. Swartz for a personal communication concerning the use of this product and to the Digestive Ferments Company for a supply of the material.

In making the medium one proceeds as already described, but ten grammes per 1,000 cubic centimetres of medium of Bacto-difco peptone is used instead of the mixture of "trypamine" and trypsinized ox-heart.

(ix) *Effect of Reduced Oxygen Tension.*—Several experiments were made to compare the growths of primary cultures at the oxygen tension of the air with those grown at reduced oxygen tension. These experiments led to the conclusion that if the medium described be properly prepared, very little indeed is gained by culture under reduced oxygen tension.

III.—Source of Material Examined.

The cases from which the strains herein discussed were isolated were cases of acute and sub-acute gonorrhœa in the male, and the majority were from men attending the venereal diseases treatment centre of the City of Dundee.

I here desire to express my thanks to Mr. D. M. Greig and to Dr. Profeit, his successor, as venereal diseases officers of this city for submitting the cases to me for examination and for every assistance in the prosecution of the work.

Cases of acute and subacute urethritis in the male of admitted venereal origin were designedly chosen as the sole source of the cultures in order to exclude as far as possible the introduction into the series of either the pseudo-gonococcus of Rosenthal or the kindred organism described by

Wollstein (1917) [21], which appears to be responsible for the causation of vulvo-vaginitis in children.

When collecting the material the patient is instructed to pull back the prepuce as far as possible; the glans is then washed with water and finally swabbed with cotton wool dipped in absolute alcohol. The meatus urinarius is now swabbed out with a small sterile "diphtheria swab" dipped in alcohol, and is finally cleaned and dried with a dry sterile swab. The urethra is then gently massaged from behind forward, and a drop of the exudate is thus squeezed forward to the meatus, where it is sucked up into a capillary bulb pipette, from which it is blown on to the surface of the culture medium, *which, it is repeated, must be heated to 37° C. before inoculation.*

The condensation water is now run over the surface of the medium and the pus is finally distributed over the surface of the agar by means of a sterile platinum loop.

In cases of acute gonorrhœa it is usual, when this technique is followed, to obtain pure cultures of the gonococcus, but in cases of some standing, secondary infection, notably with diphtheroid bacilli, is very frequent, and sometimes leads to great difficulty in isolating the gonococcus.

IV.—Cultural Characters.

On the media described, colonies of the gonococcus present the following characters: the colonies are usually discrete in primary cultures, and after forty-eight hours incubation at 37° C. are from one to three millimetres in diameter; they are slightly raised above the surface of the medium and appear to have a centre area more elevated than the margins. The surface is glistening and the colour pale grey, and when viewed by transmitted light the colonies are seen to be transparent. To the unaided eye, the colonies appear to be circular in outline, but with a lens magnifying ten to twelve diameters, the edge is seen to be scalloped, and often radial striations are noted.

After culture for some days, the features become modified and often small opaque patches of "supergrowth" develop. These have also been observed by Blair M. Martin (1911) [13] and by Hermanies (1921) [10].

The most variable feature of the colony is the degree of transparency which it exhibits, the growths tending, on the whole, to be more opaque when cultivated on media that are acid to Ph. 7·6, and more transparent on those which are more alkaline.

The most constant feature of the growth is its peculiar mucus-like quality, which is readily appreciated when a colony is picked off for isolation, for the growth tends to hang to the edge of the loop and to the agar at the same time, leaving quite an appreciable string of growth between loop and medium.

V.—Carbohydrate Reactions.

These were tested only in the case of every fifth strain isolated and the carbohydrates used were glucose, maltose and saccharose.

The glucose used was glucose puriss, Khalbaum, purchased in 1913, as also was the saccharose, while the maltose was prepared for me in 1917 by Principal J. C. Irvine, C.B.E., F.R.S., when I was engaged on a study of the meningococcus.

In making the carbohydrate tests, diluted agar, similar to that employed for stock cultures, was used, but pea-flour extract was omitted.

Tubes six inches by half inch in size, containing 2·5 cubic centimetres of the nutrient agar suitably tinted with litmus, are melted, and to each is added one cubic centimetre of a four per cent solution of the sugars. These are mixed and sterilized in the steamer for fifteen minutes, whereupon the tubes are cooled to 56° C. and one cubic centimetre of one in ten diluted human plasma is added to each.

Medium prepared thus is inoculated as a stab culture, but if required surface slants can be employed, in which case both sugar solution and plasma are added in more concentrated form to obviate over-dilution of the agar.

Fluid cultures are not so satisfactory as are growths on solid media, for, owing to the mode of preparation of the nutrient agar, contaminations are liable to occur and contamination in a solid medium is much more readily appreciated than in a fluid medium. Moreover, the growth of the gonococcus in fluid media is usually sparse and if growth fails to occur a false "negative" result is obtained—a danger which is entirely obviated by the use of solid media.

The twenty strains tested in this way all gave the classical reactions of the gonococcus—production of acid in presence of glucose but no change in maltose or saccharose.

The reaction in most instances was definite in twenty-four hours, after which no further change occurred, but some strains, apparently owing to scanty or slow growth, produced acid from glucose only after four days' incubation. I should have liked to test out the fermentative reactions of all the strains isolated and to extend the series of substrates to include numerous carbohydrates, alcohols, and glusides, but the conditions under which the investigation was conducted precluded elaboration of the work in this direction.

(*To be continued.*)

ON THE INSTINCTIVE FACTOR IN HYSTERIA.

By Major V. T. CARRUTHERS.

Royal Army Medical Corps (Retired).

Instinctive Behaviour in General.

Instinct is a great matter.—*Falstaff.*

The study of instinct may be approached from two points of view, the biological and the psychological. It will be considered here mainly from the former; but it seems practically impossible to keep the two aspects of the case permanently separated from each other. Drever [1] especially, makes the distinction between the two methods, and points out that the biological treatment of the subject deals only with behaviour, whereas the psychological province is in the experience which underlies the behaviour. He admitted, however, that the study of behaviour may be useful to verify psychological conclusions already reached, and as a secondary source of the data of psychology. He also points out that "to understand behaviour as we wish to understand it we must interpret it in psychological terms." But though psychological considerations cannot be left entirely on one side, no attempt will be made here to enter into the psychology of the intellectual processes, for the aim of this essay is to present a study in practical medicine, not in philosophy.

It is necessary to the purpose of tracing the influence of the instinctive factor in the ætiology of functional nervous disease to have a clear idea what is implied by the adjective "instinctive," in order that we may be able to recognize such behaviour when it comes under observation.

There is no lack of definitions of instinct. What Falstaff meant, and what ordinary conversation understands by the word, is, undoubtedly, knowledge that is not acquired by experience nor obtained by instruction. Drever [2] puts the same idea neatly as follows: "Apparent knowledge without experience; skill without learning; actions adapted to an end without prevision of the end; these are the characteristics of instinctive behaviour."

Romanes did not distinguish definitely between the psychological and biological points of view. His famous definition [3] runs as follows: "The name given to those faculties of mind which are concerned in consciously adapted action, prior to individual experience, without necessary knowledge of the relation between the means employed and the ends obtained; but similarly performed in similar and frequently recurring circumstances by all the individuals of the same species." Lloyd Morgan's definition seems a very complete one [4]. He says: "We are now in a position to define instinctive behaviour as comprising those complex groups of co-ordinated acts which are, on their first occurrence, independent of experience; which

tend to the well-being of the individual, and the preservation of the race; which are due to the co-operation of the external and internal stimuli; which are similarly performed by all the members of the same more or less restricted group of animals; but which are subject to variation and to subsequent modifications under the guidance of experience."

McDougall lays stress on the cognitive and affective elements in instinctive processes, hitherto somewhat neglected. He writes [5]: "We may then define an instinct as an inherited or innate psycho-physical disposition which determines its possessor to perceive and pay attention to objects of a certain class, to experience an emotional excitement of a particular quality on perceiving such an object, and to act in regard to it in a particular manner, or at least to experience an impulse to such action."

The invoking of a "disposition" has been criticized, but considering the practical scope of the "Social Psychology," probably unjustly.

Bearing all these definitions in mind it will still be found that it is not always easy to recognize instinctive behaviour at a glance. The springs of action are four: reflex, instinct, intelligence and reason. These four are in much the same state as morphological characters, inasmuch as they are easily recognized in pure or extreme examples of each, yet their lines of demarcation are somewhat hazy.

It has long been recognized, as the Peckhams [6] point out, that classification in morphology must be provisional and for the convenience of observers, who should know that between adjacent types there is a border zone where they are inextricably mixed. The same holds good in these four psychological classes.

As regards the first two, Stout [7] finds that "reflex action is of a nature fundamentally different from instinctive conduct. The difference is that instinctive does, and reflex action does not, presuppose the co-operation of intelligent consciousness, including under this head interest, attention, variation of behaviour according as its results are satisfactory or unsatisfactory, and the power of learning by experience."

This difference may be theoretically sound, but the difficulty when we come to specific cases is to know where we are to presuppose these factors, and what our presuppositions may be worth. It would appear also to allow us to say that certain actions were not reflex, but would not help us in assigning them to instinct rather than to intelligence.

As an example of a case where we are not much helped by these criteria we may mention the complicated devices by which many insects provide for their young.

As Fabre says [8] "The hymenoptera become past masters in a host of industries for the sake of a family which their facetted eyes will never behold, and which nevertheless the maternal foresight knows quite well. One becomes a manufacturer for cotton goods and mills cotton-wool bottles; another sets up as a basket-maker and weaves hampers out of scraps of flowers; the third turns mason and builds rooms of cement and

domes of road-metal; a fourth starts a pottery-works in which clay is kneaded into shapely vases and jars and bulging pots; yet another adopts the calling of pitman and digs mysterious, moist, warm passages underground. A thousand trades similar to ours, and often even unknown to our industrial system, are employed in the preparation of the abode. Next comes the victuals of the expected nurslings; piles of honey, loaves of pollen, stores of preserved game cunningly paralysed. On such works as these, having the family for their exclusive object, the highest manifestations of the instinct are displayed under the impulse of maternity."

Now all these complicated procedures have reference to a result which will never be known to the insect, therefore no variation of behaviour according to results is possible; yet attention and interest are surely present. Are these activities, then, reflexes or instincts?

Another example is in the case of the "spinal" crayfish. If one leg is seized, it is flexed and drawn up, and later if the leg is not released, all the others are brought round it and push at the hand holding the limb [9]. Here we have variation of behaviour according to results, but we have surely no right to pre-suppose interest or attention.

If we could satisfy ourselves in any given case whether intelligent consciousness was present or absent the greater part of the difficulty of the subject would disappear. But, as Jennings says [10], "By observation we cannot tell whether the reacting organism is conscious, for this would require an objective criterion of the subjective—(an objective criterion of that which is not objective)—and this is impossible." Lloyd Morgan also confesses that the boundary line is hard to draw. When trying to find a formula that shall differentiate instinct clearly from reflex he says: "If then we say that reflex acts are local responses of the congenital type due to specialized stimuli, while instinctive activities are matters of more general behaviour usually involving a large measure of central (as opposed to local or ganglionic) co-ordination and due to the most widely-spread effects of stimuli in which both external and internal factors co-operate, we shall probably get as near as possible to the distinction of which we are in search. But it must be remembered that there are cases in which the distinction can hardly be maintained." A good specimen of these cases would seem to be supplied by what is known as the "simultaneous combination" of reflexes. When a given spot on the skin of "spinal animal" receives a number of stimuli, each one of which has its appropriate response, a certain one of these responses obtains ascendancy over the others (monopolizes the final common path), and not only so but also inhibits those responses which would oppose its action. The stimuli which in this way generally dominate the situation are those which tend to damage the receiving surface, though the relative intensity of the stimuli, state as to fatigue, and other considerations have their influence" [12]. In this case of the spinal animal the measure of co-ordination and of co-operation between external and internal factors seems already fairly large, but when

we consider similar phenomena in the conscious animal, we have a state of affairs more complicated still, and thus described by Head [13]: "The simplest physical stimulus acting on the peripheral nervous system may produce different impulses which are incompatible from the point of view of sensation. These are sorted and regrouped; some are facilitated, others are repressed, before the final sum is presented to consciousness. Many different impulses remain on the physiological level—never form the basis of a sensation—they are destined to control reflex activity or to co-ordinate movements of the body and limbs."

These acts of sorting, facilitating and repressing that are necessary before a reflex or sensation shows itself, what are they but the manifestation of instinctive behaviour on the part of the centres involved?

We may conclude, therefore, that there is no hard and fast line to be drawn between reflex and instinctive action.

It may even be that the two are related in development, as J. A. Thomson says [14]: "It is conceivable that what in some cases required to begin with—it may have been for a million years—genuine behaviour, the controlled co-ordination of a chain of activities, so that they lead to an effective result, may in the course of time be short circuited and sink to the plane of reflexes, leaving the organisms disembarrassed and free for fresh adventures."

Intelligence as a factor in behaviour is easily distinguished theoretically from instinct. The difference cannot be better expressed than in Lloyd Morgan's words [15]: "Whereas instinctive behaviour is prior to individual experience, intelligent behaviour is the product and outcome of such experience."

If we keep this definition in mind we shall find no difficulty in assigning particular acts to their proper class; but when we come to consider behaviour as a whole we shall find that it is often made up partly of acts determined by heredity, and therefore prior to experience, and partly of such acts modified by experience in their performance. It is not always possible to say with certainty where the one ends and the other begins. Darwin in his observations on earthworms noted that they pull leaves into their burrows by seizing them invariably in the manner that is best calculated to facilitate their entry into the narrow opening. He summed up a large number of observations and experiments on this point as follows [16]: "As chance does not determine the manner in which objects are drawn into the burrows, and as the existence of a specialized instinct for each particular case cannot be admitted, the first and most natural supposition is that worms employ all methods until they at last succeed: namely, that worms although standing low in the scale of organization possess some degree of intelligence. This will strike everyone as very improbable, but it may be doubted whether we know enough about the nervous system of the lower animals to justify our natural distrust of such a conclusion."

If we apply Lloyd Morgan's criterion to this case, we have no possible

hesitation in classifying the behaviour as purely instinctive. It is expressly stated that the selection of the correct point in the leaf is not the result of trial and error: the "knowledge" involved is therefore inborn. If the conduct were neither hereditary nor the result of trial and error it could only be attributed to a general familiarity with mechanical principles which the life of the earthworm offers the creature no opportunity of learning, even if it had the capacity.

Fabre's description of the behaviour of the necrophorus or burying beetle [17] presents conduct that is not so easily classified as the foregoing. The habit of this beetle is to scrape out earth from under the dead bodies of small animals until they are completely buried, when they serve as a nidus for the insect's eggs and larvæ.

Fabre placed a dead mouse on a brick buried flush with the surface of the surrounding soil. A little band of necrophores began in their usual way to try and bury the carcase, but could make no progress on account of the unyielding nature of the substratum. After working in vain for some considerable time they came out from under the body and began to explore the surrounding earth. They did not do this very cleverly, apparently, since there was a lack of system and of depth in their exploratory excavations; and they soon went back under the mouse and recommenced their fruitless scraping. However, after two or three more journeys into the open they seem to have decided to move the corpse off the brick. This they attempted by all heaving the body according to each insect's private inclinations, with the result that no progress was made. At last they seemed to have hit on the plan of all working together and soon had the carrion on to soft earth and buried.

Another set of experiments consisted in tying a dead mole in various positions to sticks, so as to prevent it being buried. The beetles, after a short time of excavation, noticed that the body was not descending into its grave in the way it should, and they swarmed all over it to ascertain the cause. When the animal was held up by straw bonds encircling the body and holding it to a horizontally supported stick, they found the straws and cut them with their mandibles. When it was tied up by the hind legs they would try to cut through the limbs, and often succeeded in doing so. When the bones were too tough they left the legs and cut the straw that held them. When the suspension was done with wire, they got the body down by cutting through the legs if the bones were young and tender: but when both bones and suspenders were impenetrable the "undertakers" (croque-morts) left the affair altogether and went elsewhere.

With regard to these experiments Fabre gives the preliminary warning that to admit in the intellect of the insect the presence of a lucid knowledge of the connexion between effect and cause, the end and the means, is to make a statement of serious import, which is very well suited to the philosophical brutalities of the age. He says of the experiment with the brick that the explorations were unintelligently carried out, and that the beetles took an excessively long time to hit on the correct method.

With regard to the second set of tests he complimented the buriers, but without exaggeration, for he says that in cutting the straw they only did what they are accustomed to do when they have to dig their pits among grass-roots; and that when they were defeated by the hard bones and wire they could have succeeded by a simple manœuvre which would have dislodged the wire from its supporting twig. He appears on these counts to hold intelligence to be absent. In criticizing this opinion we must bear in mind Dr. Peckham's warning [18]: "One must be familiar with the normal conditions of the insects in question before he is able to note those slight changes in environment that offer some opportunity for an adaptation of means to ends, or before he is competent to devise experiments which will test their powers in this direction." While remembering all this and with every deference to the "insects' Homer," we cannot help thinking that all who adopt Lloyd Morgan's criterion will agree that much of the necrophorus' behaviour was the result of experience and therefore intelligent. Our judgment in this is fortified by Hobhouse's remark that "When psychologists take occasional inconsistency as proving the utter absence of intelligence they are using an argument which would equally disprove the existence of intelligence in man" [19].

If we admit the presence of intelligence are we therefore bound to agree that the insect has a "lucid knowledge of the relations between cause and effect"? It would seem that it is not necessary, nor indeed possible, for we have no means of knowing. All that need be present is an acquired association in the animal's mind between two percepts. The case is in the same class as that of Lloyd Morgan's famous chicks, which avoided pecking at bright coloured objects, which they had learnt to associate with an evil taste [20]. The difference between Fabre's estimate and that of other naturalists may be due to the fact that he does, and others do not, regard intelligence and reason as interchangeable terms.

However that may be, it is not easy in the case of this burying habit to say exactly where instinct ends and intelligence begins. If we say that the digging of the pit is instinctive, and that the heaving of the dead body off the brick on to soft ground is intelligent, what shall we say of the indeterminate movements that come between those two extremes, and accomplish nothing but the agitation of the corpse?

We seem to be quite justified in ascribing the digging to instinct, for it is performed by all animals of the species prior to experience of its results; and in ascribing to intelligence the cutting of the upper attachments, for the operation results from experience of cutting the grass-roots that obstruct the digging impulse. The whole act of burying the animal is therefore partly instinctive and partly intelligent, and in its entirety cannot be classified as one rather than the other.

It can sometimes be observed that what is a fixed and invariable instinctive act in one member of a species may become modified by intelligence in another member. For example, there is a sphex which collects crickets

for her burrow, but she does not take them straight into the hole on reaching home. She drops them at the entrance and runs forward into the nest, apparently to see if all is well inside. Then she comes out again and pulls the cricket head first into the burrow. Fabre took advantage of the wasp being inside to move the victim several inches from the hole. The wasp came out, found the body, took it to the entrance, left it there a second time and went underground again by herself to explore the den. Fabre again and again repeated the trick, and the wasp went through the performance forty times in succession, which exhausted the patience of the philosopher and he tried no more.

The Peckhams [21] tried the same manœuvre but their wasp realized the position after less than half a dozen repetitions, and took its cricket straight into the burrow without any preliminary inspection.

However much we may feel inclined on theoretical grounds to agree with Bergson's [22] opinion that instinct and intelligence are on utterly different evolutionary levels, yet in face of such observations as we have cited, I think it must be conceded that the two are closely associated in the domain of behaviour. In fact Lloyd Morgan, in the definitions of instinctive behaviour just quoted, lays it down as part of the essence of such conduct that it shall be modifiable by experiment, which is to say that it shall often be seen working in association with intelligence.

In ordinary conversation "intelligence" might perhaps be taken to include "reason"; but for the purposes of technical discussion of behaviour it is better to make a distinction between the two.

It is usual to limit the scope of reason to those actions which are prompted by a conscious consideration of the past with reference to the future; while intelligence may be attributed to any activity which is the result of experience.

In man it is generally possible to differentiate between these two by introspection and analogy, but in animals, as we have already remarked, no such certainty is attainable.

An example of reasonable conduct is afforded by the man who digs an irrigation channel through his land. He has observed that his crops die in summer if not watered. He has also observed that if he digs a canal from a neighbouring reservoir through his land, water will flow over his fields if the reservoir is at a higher level than the fields. He therefore makes his irrigation system in anticipation of reaping crops to supply his needs.

For an illustration of intelligent action in the restricted sense, we may go to the learner at a game such as golf. In the course of the game he frequently has to project the ball to a distance of, say, fifty yards when approaching a hole. By dint simply of making a great many strokes he learns to associate a certain expenditure of strength with a fifty-yard "shot"; and if after much practice he frequently employs too much or too little strength he is said to be an unintelligent player. In this case no

amount of conscious consideration of the past or anticipation of the future will help in the least. All that has to take place is a linkage between the two percepts, strength employed, and distance of the ball's flight. And yet the knowledge of the amount of strength to be applied does not come within our definition of instinct; for it is by no means inborn, but is the result of much experience.

Examples of intelligent activities are very common in animal life. Young birds peck at all objects and learn to associate certain appearances with unpleasant taste and select their food accordingly. Animals shut up in cages learn by experience that the touching of certain latches opens the door, i.e., they associate the pressure on the latch with opening of the door and freedom [23]. But it is doubtful whether rational behaviour is ever found except in man. At any rate, it is not necessary, as far as we know at present, to assume that animals have the power of consciously reflecting on past experiences with a view to shaping their conduct towards a mentally conceived goal. The negative of this assumption is, of course, hard to demonstrate; but so long as animal behaviour can be explained without the assumption, no disproof of it seems to be required. At the same time consideration of animal behaviour shows that it cannot be explained solely in terms of association of two or more percepts. Hobhouse [24] finds, for instance, that dogs are capable of (*a*) making class inferences. This he exemplifies by the fact, among others, that dogs which he observed were able to find their way out of strange houses in a few seconds, thus proving that they could deal with objects presenting a general similarity to those already known to them. (*b*) They are capable of knowledge of the object as the centre of relations. To perception any object is the centre of many relations. Experiment shows that animals are able to avail themselves of any particular relation that serves the purpose of the moment, and to be guided by desire to find out what is out of the range of perception. (*c*) Animals as well as men are capable of knowing individuals and recognizing them as such; and (*d*) some animals, especially monkeys, are able to apply their experience. While these observations show that association is insufficient to explain completely the behaviour observed, they do not seem to demand the acknowledgment of rational processes. Hobhouse asks for recognition of the fact that they imply the power to form practical judgments from concrete experience without awareness of relations as such, or of order as such, or qualities as such. Granting the usefulness of this nomenclature, or even its indispensability, do we not here seem to have arrived at a border country between reason and intelligence—especially in observation (*d*) relating to the application of experience—where only by constant effort can we keep the two territories separate in our minds?

Our survey of this interesting subject has necessarily been brief and imperfect. Its purpose will, however, have been served if it has justified the initial observations as to instinctive behaviour, namely, that though

we can define it with fair precision, yet it passes by imperceptible gradations downwards into reflex action and upwards through intelligence to rational conduct.

The Primary Instincts of Man.

When we use the adjective "primary" in connexion with the instincts, we imply that they are not always seen acting in an unmixed or pure condition; and this is certainly the case. The instincts interact on each other and the cognition of any object frequently arouses several conflicting or co-operating emotions—so that the conduct that finally ensues must be something like the algebraic sum of several impulses. It becomes, therefore, important to have some rule for analysing these complicated activities, and for enabling us to describe the elements of which they are compounded. McDougall [24] gives the principles for the recognition of the primary instincts. The first is that they shall occur among the higher animals in a clearly observable form; the second is, that they shall sometimes appear in man with morbidly exaggerated intensity, apart from such general hyper-excitability as is displayed in mania. These two principles work very well in practice, and often enable us to reduce the confusions of human conduct into clearly recognizable constituents.

The principal instincts of man thus isolated are those of fright, repulsion, curiosity, pugnacity, self-abasement (or subjection), self-assertion (or display), the parental instinct, the reproductive and gregarious instincts. Each of these instincts has its proper emotional or affective element, though the emotion of the last has no name in popular use. The emotions of the others are, respectively (according to the same author), fear, disgust, wonder, anger, subjection, or negative self-feeling, the tender emotion and love in the restricted sense. Of these instincts the most important for our present purpose are those of self-assertion and self-abasement. The others, no doubt, play their part in the etiology of functional disorders, and we shall consider some of them later, but in every-day life these two, which we may, perhaps, group as the self-regarding instincts, seem to be predominant.

The Instinct of Self-Assertion or Pride [25].—As was observed long ago by the Autocrat of the Breakfast Table, the third vowel is the natural centre of everybody's circle of life. Five minutes' conversation with some persons will give the physician knowledge of a sufficiently large sector to determine the whole circumference; while the arc of other lives is so large that it hardly differs from a straight line. The Autocrat considered that the largest intellects betrayed their circular nature the least, and the smallest ones the most clearly, and thereby invested his analogy with that justice which has become proverbial and notorious as "poetic." As a matter of fact, unless we are to consider nearly all the greatest soldiers, most poets, and artists generally, many great divines, and also several leaders of scientific progress—unless these are all to be classed as small

intellects, we must give up the idea that the underlying egotism of great men is less easily demonstrated than that of smaller natures.

The instinct of self-assertion or display is recognized on its conative side by conduct which is known to boys as "showing off," its affective or emotional aspect is known as positive self-feeling, or elation, and is pleasurable, and the instinct is evoked by success in the primary activities of life such as war, sport, love and reproduction. It is classed among the primary instincts because it is clearly observable among the higher animals and frequently occurs in man in a morbidly intense form.

Everybody is familiar with the assertive display of birds, horses and dogs—how their gait is altered, how their natural ornaments are displayed, and all other impulses, even that of hunger, neglected in order that the emotion of positive self-feeling may be expressed. We will soon consider its morbid exaggeration in man.

The chief uses of the instinct are three: (*a*) It is an accompaniment and assistant to the sex-instinct. The association between sex and display is so obvious as hardly to need mention. Clothes and courting are associated words all over the world; (*b*) In the form of personal ambition it tends on the whole to improve the individual chances of survival; (*c*) As national pride, though it sometimes brings a people to ruin, yet it generally stands for success in the international race. Without it there would be no continuity in plans for tribal improvement, and no cohesion in efforts for the protection of the fatherland. It is only to be expected that an impulse of such importance should sometimes show itself in exaggerated morbid forms. These forms are familiar to all in such diseases as general paralysis of the insane, and in that psychosis of youth known as pseudolalia fantastica, in both of which conditions the "showing off" consists in boastful false accounts of the subject's physical or mental powers. Morbid exaggeration too, though less within the domain of disease, is often seen in the extravagant fashion in dress, or freakish ornamentation of the person so common in all races and times. Queen Elizabeth's wardrobe, in 1600, consisted of 1,075 dresses and mantles of different kinds without counting her coronation and parliamentary robes. Moreover the fashions of the period showed almost incredible folly in their vagaries. "The ruffs were one of the most monstrous fashions of the times. They were worn by men and women alike, and were made of the finest lawn or cambric. They were at least a quarter of a yard deep, and were made to stick out stiffly round the neck either by being starched or by being supported with an elaborate arrangement of wires. Stowe, a historian who lived at that time, says that he was held to be the greatest gallant or beau who had the deepest ruff and the longest rapier. At last Elizabeth had to place grave, selected citizens at every gate to cut the ruffs and break the swords of all passengers if the former exceeded a yard wanting a nail, in depth, or the latter a full yard in length" [26].

One of the greatest of Napoleon's generals, Murat, was curiously

attired during a formal entry into Warsaw. "His charger's bit and stirrups were of gold, his saddle-cloth a tiger's skin and he himself was ablaze with gold and colour. He wore red leather riding boots, white breeches, a tunic that showed only a mass of gold embroidery, a diamond hilted sword of scimitar shape suspended by a jewelled baldrick, a pelisse and shako of costly furs, the latter with ostrich and egret plumes held by a diamond clasp" [27]. To prove that this was an abnormal extravagance it is recorded that Napoleon refused to see him while thus dressed "like a circus rider" and made him change into more soldierly kit. The study and homage devoted to "keeping up appearances" that makes slaves and malcontents of so many other otherwise blameless souls is due to morbid exaggeration of this instinct. The highest aspirations and the most fundamental conditions of happiness are ruthlessly sacrificed to it among civilized people. This has never been more trenchantly exposed than by Thackeray in his "Book of Snobs." The sordid pre-occupation and pitifully-comic tribulations of the Ponto family illustrate it well, and he justly classes such miseries together with the Chinese habit of foot-compression. "You stuff," he says, "the little rosy feet of a Chinese young lady of fashion into a slipper that is about the size of a salt cruet and keep the little toes there imprisoned and twisted up so long that the dwarfishness becomes irremediable. Later the foot could not expand to the natural size were you to give it a washing-tub for a shoe, and for all her life she has little feet and is a cripple." Nor did he overlook the horrible restrictions on marriage that were the bane of middle-class existence then as now. "And here's the wreck of two lives, mused the present snobographer, after taking leave of Jack Spigott. Pretty Mary Lovelace's rudder lost, and she cast away, and handsome Jack Spigott stranded on the shore like a drunken Trinculo. What was it that insulted Nature (to use no higher name), and perverted her kindly intentions towards them? What cursed frost was it that nipped the love that both were bearing, and condemned the girl to sour sterility, and the lad to selfish bachelorhood? It was the infernal Snob tyrant who governs us all, who says: 'Thou shalt not love without a lady's maid: thou shalt not marry without a carriage and horses: thou shalt have no wife in thy heart and no children on thy knee without a page in buttons and a French bonne: thou shalt go to the devil unless thou hast a brougham. . . . Wither poor girl, in your garrett; rot, poor bachelor, in your club.'"

The perverted emotion of positive self-feeling in this mean style has formed the motive of great fiction no less than the emotion of love: Meredith's "Evan Harrington" and Balzac's "Le Père Goriot" being good examples. Under forms equally distorted, but perhaps less mean, it is known as ambition, and has inspired Shakespeare's characters of Macbeth and Coriolanus. The instinct of display rules Mark Antony even in the face of death; for in the same breath that he calls for Eros's sword-thrust he apostrophises Cleopatra's spirit with this bombast:—

"Stay for me:
Where souls do couch on flowers we'll hand in hand,
And with our sprightly port make the ghosts gaze,
Dido and her Aeneas shall want troops.
And all the grove be ours."

It is therefore plain that this instinct is of fundamental importance in human life, sometimes proving itself the rival even of love and hunger: that it is closely linked with the preservation of the individual and the race; and that it is liable to aberrations which may influence human conduct in a variety of unsuspected ways.

The Instinct of Self-Abasement with its emotion of negative self-feeling or subjection shows itself in crouching, cringing, or slinking attitudes of body or (in the civilized) of mind. The pain may be either physical, such as that experienced in surgical operations, corporal punishments or accidental injuries, or mental, such as is produced by failure in the primitive human occupations of love and reproduction, sport and war: and the fear may be that crude terror experienced in the shadow of death, or that vague uneasiness felt by a man in the presence of those who are stronger than himself. Its object from the point of view of the community is the subordination of the private personality to the general good which shows itself as respect for public opinion, and which is so necessary to the cohesion of society.

Cassio's despair (in the play of Othello), when he thought his reputation lost in a drunken brawl, shows how strong a force this regard for public opinion may be. Its object, as far as the individual is concerned, is the propitiation of a superior power; and this easily passes into the attempt to gain sympathy or pity. The conative side of the instinct is seen in man in the bowing and scraping, doffing of hats, curtseys, exordiums and perorations of letters, etc., all of which originate in the fear of, or desire to propitiate, a superior personality. Similar states of mind occur as the result of receiving an injury. The writer has known an Asiatic fall on his knees and salaam repeatedly on being accidentally shot by a soldier on the range. And the following note appeared recently in a newspaper: "Thank you, my lord, and good luck to you!" said a man sentenced to six months' hard labour for housebreaking. Everybody, moreover, must be familiar with the change that occurs in a clever, but lazy, school-boy from conceited idleness to humble and efficient industry, as the result of a good caning.

The instinct is easily recognized in some higher animals, especially in dogs. As McDougall excellently describes it [28]: "The nature of the instinct is sometimes very completely expressed in the behaviour of a young dog on the approach of a larger, older dog; he crouches or crawls with his legs so bent that his belly scrapes the ground, his back hollowed, his tail tucked away, his head sunk, and turned a little to one side, and so approaches the imposing stranger with every mark of submission."

To the other test of a primary instinct—that occurring in morbidly exaggerated form in man—this instinct exactly conforms. Exaggerations of humility leading the patient to believe he has committed the unpardonable sin, etc., are too well known to need further reference; but apart from insanity exaggerations amounting to aberrations of the instinct are frequently met with. Henry II in his fear of the Pope after the murder of Thomas à Becket is said to have walked bare-footed to Canterbury Cathedral, and to have lain all night without food, and to have caused himself to be scourged on the bare back by all the Bishops present. S. Julien l'Hospitalier [29] forsook a gilded palace of pleasure to live alone in a mud hut on the shores of a northern firth, where he slept on dried leaves, and ate no meat, where mosquitoes made the brief summer intolerable, and dreadful frosts killed all but the strongest in winter. And, these hardships being insufficient to appease the instinct of subjection, he finally stripped and lay skin to skin with a leper and so lost his life. In the East, excess of negative self-feeling induced by religious exercises is common. Juggernaut is, by this time, a household name in Europe, and other forms of self-immolation are numerous in India. But it should be noted that though such exhibitions begin as negative self-feeling, yet by the time they reach such extremes as to compel the sufferer to commit suicide in public, they have probably undergone some transformations.

Moderate excitation of the instinct seems to give pleasure, especially to women. There can be no doubt that the maiden who proclaims herself as less than the dust beneath the chariot wheels of her beloved, experiences no small satisfaction from so doing. The congregations also of certain churches or conventicles seem to derive satisfaction from abasing and vilifying themselves, just as the Arab does from his prostrations at sunset. But if the stimulus to this instinct exceeds moderate limits then the emotion of negative self-feeling which is aroused becomes unpleasant in its intensity and ultimately may be quite intolerable. The way of escape from this which the sufferer frequently adopts is to indulge in acts of self-display which arouse the emotion of positive self-feeling, which neutralizes the painful self-abasement that has hitherto existed. It is probable that this was the origin of rending of garments, putting dust on the head, etc., which are still seen in the East. I would suggest also that funeral pomps were instituted to assuage the negative self-feeling aroused by the death of prominent tribesmen: and there can be little doubt that the Indian who is swung by hooks passed through his flesh, above the heads of an admiring crowd, finds in that admiration ample redress for the depression that his pangs must cause him.

(*To be continued.*)

MALARIA AND MOSQUITOES IN BELIZE, BRITISH HONDURAS.

By Captain C. C. G. GIBSON.
Royal Army Medical Corps.

A study of the incidence of malaria, and of the prevalence of mosquitoes in one of our little-known possessions on the Central American littoral may be of some interest to our readers.

No troops had been stationed in British Honduras since 1886 until a detachment of the Royal Sussex Regiment was quartered there from December, 1919, to July, 1920, when the garrison duties were taken over by a company of the 1st West India Regiment. The old barracks or military area of Belize is situated on the Carribean Sea at sea level, south of Yucatan, and 660 miles west of Jamaica. This military area is limited on the north and east by the sea, on the south by the City of Belize, and on the west by very low-lying, swampy ground extending for several miles. The water of this extensive swamp is brackish near the sea, but farther inland is ordinary fresh water, and is constant for about nine months of the year. The swamp begins to dry up about the middle of May, and by the end of June has almost completely disappeared, except for a few deeper pools which remain for a considerable time. The rain commences to fall at the end of August; and this low-lying ground again becomes a swamp. As the ground becomes inundated, numerous small fish begin to make their appearance throughout the whole swampy area. It is interesting to note that certain of these pools abound with these fish, while others which are in close proximity are quite devoid of these useful aquatic inhabitants. In some cases it is difficult to conceive how these fish first got to the pools, some of which are free from communication with all other waterways. It is possible that the spawn may have remained dormant in the dried mud from the previous year, or the ova may have been transferred artificially on the feet of birds and insects, or by other agents, when the pools began to form.

The barrack rooms, three in number, situated at the north end of the cantonment, are composed of wood, with tiled roofs, and are raised about eight feet from the ground on thick wooden piles. These rooms could comfortably accommodate about sixty men each. They have wire-gauze windows and doors, but these only act as traps for mosquitoes. All the woodwork is warped and sagged, leaving numerous holes and crevices which allow all kinds of flies to enter the rooms.

The hospital, officers' quarters and canteen are situated to the south of the barracks, facing the sea, on a line varying from 10 to 100 yards from the water's edge. These buildings are ordinary square houses divided into

four or more compartments by boarding which does not reach the ceiling, and are raised about four feet from the ground.

The parade square and garrison sports ground, which is also used by the civil population, are situated in front of the lower or southern quarters. This is the only piece of dry ground in the wet season, and even this may be under a foot of water.

The sanitation of the barracks was apparently sufficient, if not up to modern tropical requirements. All rubbish had to be burned in incinerators, which was often difficult in the wet season.

Ablution was carried out in sheds situated over the sea, about twenty yards from the water's edge.

The latrines were similarly situated and in direct communication with the sea, where numerous cat-fish quickly disposed of the dejecta. In the hospital and officers' quarters a small septic tank system was in vogue.

In the City of Belize there were no moderm sanitary arrangements. All rubbish was collected in the yards, and its removal was somewhat irregular, thus leading to accumulations of dirt. All latrine excreta, with few exceptions, were collected in iron pails, and dumped into one of the canals during the evening by the individual residents concerned. The city was divided into two districts by the Belize River. Each of these districts in turn was drained by a canal, which was almost stagnant, full of refuse, and swarming with cat-fish. Originally pumps were employed for emptying these channels, but without success.

The only water supply was from rain which was collected in iron or wooden tanks attached to each building. This source of supply was quite insufficient, except in a wet summer, even if used for drinking purposes alone. The shore water was shallow, had a muddy bottom, and was contaminated for over a mile from the beach, which rendered it quite unsuitable for washing purposes. In barracks we were more fortunate owing to the large water-collecting area of the roofs. Thus the cleansing of feeding and cooking utensils could be carried out with greater safety and freedom from contamination.

There are no springs in Belize, which fact probably has some relation to the local saying that "the city is built on bottles, and is only a raised swamp."

Although sanitation is not very satisfactory, the general health of the city cannot be regarded as bad. Malaria is prevalent. A few cases of pulmonary tuberculosis are always present, with sporadic cases of both forms of dysentery at times.

The climate is more temperate than is in keeping with the latitude of the colony. During the day the direct rays of the sun are often obscured by low dark clouds. In the evening, during the winter months, a coat has to be worn, and during the night two or three blankets are required. The temperature is regulated by the wind which averages eighteen miles per hour, and which sometimes continues for two or three months without

ceasing. The prevailing wind is mostly from the north-east, although occasionally coming from the north or east. At other times it dropped for varying periods, when life was unbearable, owing to the heat, which was intense. The atmosphere was muggy and almost stifling. During these calm spells, all wearing apparel was soaked with perspiration. Added to this, mosquitoes and sand-flies appeared in swarms and continually annoyed one. The sand-flies were present during the day and night, and the mosquitoes during the evening. During the high winds, which had no relation to the season of the year, mosquito nets were not required, and one had a comparative rest from these blood-sucking pests. During the calm hours, ordinary mosquito nets were useless as a protection against the attacks of such small insects as sand-flies, and only those made of muslin were the least use against the ravages of these minute vampires. When a muslin net is used, the atmosphere within it is almost unbearable, unless an electric fan can be fixed up inside.

Both these diptera, anophelines and culicoides, were much worse at the barracks, owing to the presence of the swamps and old moth-eaten wooden posts which afforded them ample seclusion. During the interval of calm, culicoides were so troublesome that many wore gloves, and a towel or some other covering over the head and neck. Soldiers could be seen completely covered up, eating their meals and walking up and down the sea-shore to get as far away as possible from these irritating pests. At night the men would congregate in the bathing sheds over the sea, where they would often be seen crying in their misery. To make the night more trying, numbers of bull-frogs, croaking lizards, crickets, and other reptiles, which seemed to be silent during the day, made rest impossible. At other times a mild westerly breeze wafted clouds of mosquitoes, mostly culex, from the swamps, which invaded one's quarters or attacked one when in the open.

Adult anophelines were found in all permanent buildings, provided that there was not too much light, and that the draught was not excessive. Both stables and harness rooms yielded great numbers of anophelines. Leather has a potential attraction for mosquitoes, and on several occasions it was found difficult to dislodge these insects from the harness rooms. The local grooms and men who have to remain about the stables for any length of time, have a habit of lighting cocoa-nut husks and allowing them to smoulder, when the smoke drives the culicines away.

In the sleeping quarters all men were supplied with sand-fly nets, which they carefully used during the calm weather. It was found that unusual care had to be taken when using these nets during the absence of any wind. The men noticed that the moment any part of the naked human integument came in contact with the curtain, they were immediately bitten by these minute insects.

The range of flight for anophelines with no wind is up to 400 yards, but

usually only 300. With a wind they can often fly three miles, since on several occasions ships (United Fruit Company) have picked them up when that distance out. This had been repeatedly reported to the Principal Medical Officer, Colonial Medical Service, British Honduras. The longest flight known was four miles, in a wind, when it was reported from a British war-ship stationed four miles out, for some time before entering the harbour, that mosquitoes had been captured on board, which on examination proved to be anophelines.

Throughout the year, several blood-sucking dipterous insects were captured. These proved to be anophelines, culicines and ceratopogoninæ, In the warm weather, female mosquitoes were more common than male, while in the cooler season males seemed to be equally distributed.

During the still, calm nights, ceratopogoninæ were a real pest; nothing except thick muslin or thin calico could protect one from being bitten by them. They regularly attacked in swarms, and gave one no peace or rest.

The following specimens were identified:—

Anophelinæ: *Anophelina vestitipennis*, *A. albimanus*, *A. crucians*.

Ceratopogoninæ: *Culicoides maculithorax*, *C. guttatus*, *C. sp.* near *guttatus*.

During the daytime, adult anophelines were caught by placing a little cotton wool with a few drops of chloroform in the bottom of a test-tube, and placing it over the nesting mosquito. These were chiefly found in occupied quarters, along the upper part of the wall, behind pictures, clothes, etc., preferably dark clothes; a great number were also found in the stables, where the horses suffered badly from them. A few were caught in cobwebs, but only in deserted ones, as the spiders seemed to relish them. If a mosquito and a fly were placed artificially in a cobweb, the spider always devoured the mosquito first, generally leaving the fly untouched.

Some could always be found on the water tank supports, probably attracted there by small collections of water from the many minor leakages. At night there was no difficulty in capturing mosquitoes, as they were continually attacking one, and resting on the hands, head and ankles. After retiring to bed for some minutes, numbers would collect on the outside of the mosquito curtain, where they were easily captured.

Breeding places of anophelines are distributed throughout the whole military area, and consist of divers pools which form on the grass. The whole cantonment was studded with numerous land-crab holes, which were found to be a safe and undisturbed breeding-ground for mosquito larvæ.

The stumps, forks, and hollows of old trees were also found to contain many larvæ. All breeding places had certain things in common; the water was clean and often motionless, during some part of the day it was partly exposed to sunlight, and associated with a good deal of shade and some form of decaying vegetable matter. All collections of water not

naturally protected by fish, such as in crab-holes and stumps of trees, were carefully oiled twice weekly.

Throughout the whole area in the average pool formation, the Barbadian cyprinodont fishes known as "millions" were to be found. These useful little fishes played an important part in the diminution of mosquito larvæ. Water beetles were also found to ingest mosquito larvæ in large numbers. A few experiments were conducted by placing equal numbers of larvæ and nymphæ in a basin with one water beetle, "dytiscidæ," when it was noticed that the larvæ were immediately devoured by the water-beetle, whereas the nymphæ were left undisturbed, and duly hatched out the young adult mosquitoes.

Frogs and newts were not found to destroy mosquito larvæ and nymphæ when placed in captivity. Dragon flies were often noticed to enter verandahs in pursuit of mosquitoes, and their larvæ are also a potent natural enemy to mosquito larvæ. Lizards were occasionally seen stalking mosquitoes on the verandah walls.

The commonest type of malaria was benign tertian. Malignant tertian was uncommon amongst the troops, and quartan was not observed. The blood specimens of all cases of fever suggestive of malaria were systematically examined for malarial parasites, and they were generally found to yield positive results. In a few cases of high continuous fever, clinically characteristic of malignant tertian, no malarial parasites could be demonstrated, nor were crescents found at a later date. The most common fever, benign tertian, was mild and non-toxic, unassociated with complications and rapidly yielded to quinine treatment administered by the mouth. The fever rapidly fell to normal, so that the patient could be put on convalescent exercise and a prolonged course of quinine outside the hospital.

In hospital the non-European troops were given the following medicinal treatment:—

On admission, calomel, followed in four hours by sulphate of magnesia. Quinine sulphate, fifteen grains, in acid solution, followed by a similar quinine mixture containing five grains of quinine, three times daily for one week, and two and a half grains three times daily for a fortnight. This yielded excellent results, and very few relapses were observed. The intractable cases were treated intra-muscularly with quinine di-hydrochloride. All the blood specimens of the infected cases were examined weekly for the presence of malaria parasites.

All cases that had been infected were segregated as much as possible and were supplied with mosquito nets.

From December, 1919, to June, 1920, the cantonment was occupied by a detachment of the Royal Sussex Regiment numbering 250. The monthly incidence of the disease was as follows:—

December	6	May	6
January	17	June	5
February	41		
March	45	Total	143
April	23		

Percentage of troops infected: 57·2 per cent.

From June, 1920, to October, 1921, the cantonment was garrisoned by a detachment of the 1st West India Regiment numbering 125. The monthly incidence of malaria was:—

July	9	March	4
August	5	April	2
September	5	May	2
October	18	June	2
November	19	July	0
December	22	August	3
January	11		
February	4	Total	106

Percentage of troops infected: 84·8 per cent.

The Royal Sussex Company received a prophylactic issue of ten grains of quinine sulphate weekly.

The Welsh India Regiment detachment was given ten grains of quinine sulphate twice weekly.

Although no control or comparative test was established, I believe that the issue of quinine considerably diminished the malaria incidence.

The average monthly splenic index for the year 1921 amongst the non-European coloured population was as follows:—

Indians .. .. 76 per cent. Creoles .. .. 48 per cent.
Negroes .. .. .. 20 per cent.

From these figures it can easily be realized what difficulty was experienced in protecting troops in Belize from malaria, as all the necessary agents were present in a potent form, including the rainfall and general climatic conditions.

THE PSEUDO-GLOBULIN GROUP. RELATIONSHIP TO AGGLUTINATION, FLOCCULATION, AND COLLOIDAL MASTIC REACTIONS.

By Major C. H. H. HAROLD.

Introductory.

Previous work [1] has indicated that the eu-globulin group is very intimately concerned with the Wassermann reaction, and that the pseudo-globulins are mainly responsible for hæmolytic reactions. It is also established that antitoxic properties are chiefly confined to the pseudo-globulin group. On considering these facts it would appear that different diseases are capable of producing changes in different fractions of a serum and thus give rise to specific reactions.

As the agglutination response of a serum does not always bear direct relationship to the immunity of an individual (e.g., a case of typhoid fever who relapses despite a high agglutination titre), and as the Sigma reaction readings of certain cases of syphilis are not invariably positively correlated with the Wassermann reaction results, it was thought that an investigation into the particular fractions concerned with these phenomena might indicate whether a disease is capable of affecting two different fractions of a serum concurrently and so gives rise to two readings of different intensity, or, if more than one body is produced in any one particular fraction in varying relative amounts. With these points in view experiments were carried out on the lines previously employed in investigating the eu-globulin group.

Agglutination Tests.

Experiment 1.—The serum of a rabbit immunized against *Bacillus typhosus*, after inactivation, was split up into its component fractions by the successive employment of the acid dilution method, half saturation and three-quarter saturation with ammonium sulphate, and the resulting precipitates were dissolved in volumes of saline equal to the original bulk of serum. The whole serum and the various fractions so produced were now put up against an Oxford standard agglutinable culture, employing Dreyer's technique, with the following results:—

Whole Rabbit Serum.

Tubes	1	2	3	4	5	6	7	8	Control
Readings	Total +	Total +	Total +	Total +	Total +	Total +	Stand.*	Nil	Nil

* Standard agglutination 1 in 2,500.

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	Control
Readings	Stand.	Stand.*	Trace	Nil	Nil	Nil	Nil	Nil	Nil

* Standard agglutination 1 in 50.

Pseudo-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	Control
Readings	Total +	Total +	Total +	Stand.	Stand.	Stand.*	Nil	Nil	Nil

* Standard agglutination 1 in 1,000.

Albumen Fraction.

Tubes	1	2	3	4	5	6	7	8	Control
Readings	Total –	Total –	Stand.	Stand.*	Nil	Nil	Nil	Nil	Nil

* Standard agglutination 1 in 250.

Remarks.—The indication afforded by the above results is that the pseudo-globulins chiefly are concerned with the agglutination phenomenon.

Sigma Reaction Tests.

Experiment 2.—A syphilitic serum, after inactivation, was treated in the same way as the serum in Experiment 1. The resulting precipitated fractions were dissolved in saline and flocculation tests, employing Dreyer's technique, carried out with the whole and individual fractions separately.

Whole Serum.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Nil	Nil	Nil	Stand.	Stand.	Total –	Total –	Stand.	Trace	Nil

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Trace	Trace	Trace	Nil	Nil	Nil	Nil	Nil	Nil	Nil

Pseudo-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Total –	Stand.	Stand.	Stand.	Nil	Nil	Nil	Nil	Nil	Nil

Albumen Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Total –	Trace	Trace	Trace	Nil	Nil	Nil	Nil	Nil	Nil

Remarks.—The eu-globulin fraction had been dissolved in an amount of saline equal in bulk to the original serum ; the pseudo-globulins and albumens in double this amount. The pseudo-globulin fraction gave the highest reading. The eu-globulin fraction gave practically no result, and the albumen fraction also a very low reading.

Experiment 3.—An inactivated syphilitic serum was split up into its component fractions by successively saturating it one-third, one-half, and three-quarters with ammonium sulphate. It was realized that the division between the eu-globulin and pseudo-globulin groups would not be so clean cut as when employing the acid dilution method. The resulting precipitates were dissolved in a bulk of saline equal to that of the original serum.

Whole Serum.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Nil	Nil	Nil	Stand.	Total –	Total –	Total –	Total –	Stand.	Nil

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Trace +	Nil	Nil	Nil	Trace	Stand.	Trace	Nil	Nil	Nil

Pseudo-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Nil	Nil	Trace	Total –	Total –	Total –	Stand.	Trace	Trace	Nil

Albumen Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Trace	Nil	Nil	Nil	Nil	Nil	Nil	Nil	Nil	Nil

Remarks.—The albumen fraction gave a very low reading. The eu-globulin readings were inferior to the steady readings given by the pseudo-globulin group.

Experiment 4.—Experiment 2 repeated, but as the albumen fraction had given practically negative results only the eu-globulin and pseudo-globulin fractions were recovered and examined. Unfortunately, some three-quarters of the pseudo-globulin fraction was lost during the experiment, owing to the shattering of a centrifuge tube.

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Nil	Nil	Nil	Total –	Total –	Stand.	Trace	Nil	Nil	Nil

Pseudo-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Stand.	Total –	Total –	Total –	Stand.	Trace	Nil	Nil	Nil	Nil

Remarks.—The eu-globulin reading was very similar to that obtained in Experiment 3. From the pseudo-globulin group a fairly steady reading was obtained, in spite of the loss of three-quarters of the total quantity.

Experiment 5.—Repeated Experiment 4.

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Stand.	Stand.	Trace –	Trace –	Trace –	Stand.	Trace	Nil	Nil	Nil

Pseudo-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Nil	Nil	Total –	Total –	Total –	Total –	Trace	Nil	Nil	Nil

Remarks.—The eu-globulin readings were fluffy. Major Lambkin was of opinion that the "Standard" in tube 6 was attributable to contamination. In any case the readings showed mainly "Trace" reactions in the higher dilutions. The pseudo-globulin group gave steady readings throughout.

Experiment 6.—The serum of a case of florid syphilis was treated in the same way as in preceding experiments. The eu-globulin group alone was utilized.

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	Control
Readings	Nil	Nil	Nil	Nil	Nil	Nil	Nil	Nil	Nil	Nil

Remarks.—The readings are comparable to those obtained in Experiment 2.

Colloidal Mastic Tests.

Experiment 7.—The fractions of the serum obtained in Experiment 3 above were put up as colloidal mastic tests.

Albumen Fraction.

Readings throughout were "Nil."

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	10
Readings	0	0	5	5	5	4	0	0	0	0

Pseudo-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	10
Readings	0	0	4	4	3	0	0	0	0	0

Experiment 8.—The fractions obtained in Experiment 4 above were put up as colloidal mastic tests.

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	10
Readings	1	1	4	5	5	5	4	4	4	1

Pseudo-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	10
Readings	1	5	5	5	5	5	5	5	5	2

Remarks.—The striking point about this test is the fact that three-quarters of the pseudo-globulin having been lost through shattering of a centrifuge tube, the apparent effect of increasing the dilution of pseudo-globulin has been to increase the strength of its reaction.

Experiment 9.—The fractions obtained in Experiment 5 above were put up as colloidal mastic tests.

Eu-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	10
Readings	4	3	3	3	4	4	4	4	4	4

Pseudo-globulin Fraction.

Tubes	1	2	3	4	5	6	7	8	9	10
Readings	0	1	2	3	4	4	4	4	4	4

Remarks.—The pseudo- and eu-globulin fractions gave very similar readings.

Conclusions.

(1) Agglutinins are contained, chiefly, in the pseudo-globulin group.

(2) The dominant fraction in the Sigma test is the pseudo-globulin group, which has given regular and steady readings throughout. The albumen fraction gave very low readings in Experiments 2 and 3. The eu-globulin fraction gave practically negative readings in Experiments 2 and 6, and inferior readings, comparatively speaking, in the other experiments.

(3) Both the pseudo- and eu-globulins appear to be equally effective as precipitants in the colloidal mastic tests, and the actual quantity of globulin present is of considerable importance. Variations in this may give quite unexpected results, as in Experiment 8, where a high reading was obtained though only a quarter of the pseudo-globulin content of a serum was utilized. The colloidal mastic reaction is apparently only indicative of the presence of globulins.

Acknowledgments.

I desire to express my indebtedness to Major A. T. Frost, O.B.E., R.A.M.C., O.C. Military Hospital, Rochester Row, for providing me with the facilities and material for carrying out this investigation; and to Major E. C. Lambkin, D.S.O., R.A.M.C., Pathologist to the above hospital, not only for his active co-operation and for the performance of the Sigma and colloidal mastic reactions, but for his personal interest in the investigation.

Reference.

[1] Journal of the Royal Army Medical Corps, August, 1922.

Clinical and other Notes.

NOTES OF THREE INTERESTING CASES OCCURRING IN THE BRITISH CORPS OF OCCUPATION, CONSTANTINOPLE.

By Major H. J. BENSTED.

Deputy Assistant Director of Pathology, British Corps of Occupation, Constantinople.

Case I.

Serjeant E., R.F.A., was admitted to hospital late at night with a history of of having been shot[1] in the chest some two hours before.

The patient was very little shocked from the journey and after rest in bed with warm applications his condition was very fair. He complained of a pain in the right side of his chest, but was not seriously troubled by it. On examination a small wound—less than ¼ in. in diameter—was seen in the anterior axillary line in the eighth interspace on the left side of the chest. The bullet could be felt at the back, on the right side, just under the angle of the scapula, lying in a subcutaneous position. There was no other external sign of wound or injury. It was impossible to tell the direction of the bullet in the body as it had hit the rib above, but it appeared to have started in an upward direction. The pulse and temperature were normal, the whole of the abdomen moved evenly and well and there was neither superficial nor deep tenderness on palpation. The bowels had not moved since the wound had been received, but there had not been any vomiting, and apart from the pain in the chest the patient was quite comfortable. The wound was treated, but nothing further was done. The patient passed a very fair night and was no worse the next morning. The condition remained *in statu quo* until just past midday when the pulse rate began to rise and vomiting commenced, and for the first time the patient really looked ill. Rigidity was noticed in the upper left abdomen and the blood-pressure was falling. Immediate operation was advised. The abdomen was opened by a vertical incision in the outer upper left rectus. A large amount of free blood was seen in the abdominal cavity and investigation showed that the bleeding came from a torn vessel in the omentum. The transverse colon was also wounded in two places and a large blood clot was seen at the bend. The wounds were repaired, but the clot was not touched. A large rubber tube was inserted down to the clot and the skin wound closed round it. A wide-bore suprapubic drain was also inserted. The patient's condition was extremely critical and he never really rallied from the operation, and he died before eight o'clock that night. His temperature had never been above normal and it was markedly subnormal just before death. He lived for twenty-two hours after being wounded, and for the first fifteen or sixteen hours showed no signs or symptoms of a serious wound.

The bullet was removed through a subcutaneous incision at the time of the major operation.

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[1] The bullet was fired from a small automatic rifle ·22 at a range of about ten yards.

The post-mortem was performed the following morning. The original wound and the surgical wounds, were found as described above. The body was opened and it was found that the bullet had penetrated the pleural cavity at its base so that beyond a slight contusion the lung was not damaged. The diaphragm was perforated immediately opposite the pleural wound. There was some old pleurisy on the right side and there was some hypostatic congestion of both lungs. Beyond this the whole of the thoracic contents were normal.

There was a considerable amount of free fluid in the abdominal cavity with an acute general peritonitis. The fluid was bloodstained.

The liver and spleen were undamaged and quite normal. The great omentum was packed round the sutured wounds in the colon, and as this was being rearranged very definite areas of fat necrosis were seen. Practically the whole of the pancreas was destroyed and the portions were almost unrecognizable in a mass of blood clot, which already was infected from the bowel.

The bullet having passed through the pancreas, entered the perirenal fat without damaging the kidney, and buried itself in the thick muscles of the back and then in some manner passed between the spinous processes of the tenth and eleventh vertebræ to the other side and finally came to rest in the subcutaneous tissues where it was found.

The whole of the stomach and intestines were carefully examined, but beyond the sutured wounds described, no other damage was found. The kidneys and suprarenals were normal,

Cause of death: Gunshot wound of abdomen perforating a hollow viscus and causing destruction of the pancreas which give rise to an acute hæmorrhagic pancreatitis. Immediate cause of death, toxæmia from the pancreatitis and general peritonitis from the perforated colon.

(Clinical notes supplied by Major J. Dunn, F.R.C.S., R.A.M.C., Surgical Specialist to the Command.)

Case 2.

Boy, G. H., R.N., who was found dead in his hammock in the morning.

There was no apparent cause for death, his history sheet was clear and externally no abnormality could be found. The only history of any value was that the evening previously he had eaten a whole tin of salmon just before turning into his hammock, and that there had been some bad tins in the batch in the canteen.

The body was opened and the organs examined in the following order: thyroid gland: normal; œsophagus: normal; trachea: normal; larynx: normal; aorta: normal; thymus: very much enlarged, quite three times the size of the gland seen in a normal infant; bronchial glands: pale and enlarged; cervical glands: pale and very definitely enlarged; lungs, etc.: pleura normal; lungs small, good colour and normal; heart: slight excess of pericardial fluid; muscle good and no other abnormality; liver: enlarged and blood dripping; spleen: enlarged and very flabby, malpighian bodies enlarged and standing out clearly: kidneys: congested; pancreas: normal; adrenals: normal; stomach, etc.: empty to the beginning of the ileum and thence contents were normal; the stomach was intensely congested, but there was no loss of mucous membrane seen in any

part of the congested stomach or duodenum; all the lymphatic tissue in the abdomen was hypertrophied; brain, etc.: normal.

Cause of death: status lymphaticus. Cause of stimulation: (?) toxæmia from tinned food.

N.B.—There was no evidence as to whether there had been any vomiting before the patient went to his hammock.

CASE 3.

An English lady of 75, non-inoculated, was ill three weeks in her home with slight diarrhœa. A Greek doctor saw her, diagnosed acute T.B. lung and eventually sent her into a women's and children's hospital. Just before she died I saw her with the medical officer, and we both thought of typhoid. I took a blood culture on chance and got a very good growth of *Bacillus typhosus* in thirty-six hours. Fæces also positive; Widal negative. Temperature subnormal until just before she died.

TWO CASES OF LEUKÆMIA.

By CAPTAIN D. POTTINGER, M.C.
Royal Army Medical Corps.

Case 1.—Sapper X, aged 22, was admitted to Fort Pitt Military Hospital on February 2, 1922.

Family History.—Nine brothers and sisters. Two brothers killed in the war. All other members of the family alive and healthy, including father and mother.

Condition on Admission.—Stated he had been on full duty previous to admission but lately felt a bit "off colour." Complained of headache and sore throat of twenty-four hours duration. Temperature 100° F., pulse 76. Pain in left side and slight cough. Slight friction rub could be heard. Lymphatic glands in anterior triangle of neck were moderately enlarged and tender.

Stated that he had noted a similar "lump" some weeks before but it had gone away of its own accord. On third day of admission moderate epistaxis occurred. Pain in the left side continued, and the painful site was painted with iodine. Temperature continued at 100° F., and pulse rate 80.

The counter-irritation of the iodine relieved the pain but produced a purpuric rash exactly coinciding with the area painted: a similar rash, but very much milder in type, appeared on the forehead.

Blood examination at once cleared up the diagnosis, showing: red blood corpuscles, 1,375,000; leucocytes, 268,000; hæmoglobin, twenty-six per cent.

Laboratory Report.—Blood picture was typical of an acute lymphatic leukamia. Lymphocytes exceed ninety-nine per cent and are very irregular in size and shape of nuclei. No myelocytes or eosinophils were seen, and no nucleated red cells.

The progress of the case was rapidly down hill. The epistaxis continued and was more severe. Other lymphatic glands in the neck, axilla and groins became enlarged. Temperature varied from 100° to 104°, rising towards the termination of the illness. Pulse rate from 104 to 136.

Patient died on February 12, 1922. Anæmia, not at all marked on admission, rapidly increased as the case progressed.

Autopsy.—Spleen much enlarged, friable and adherent to the diaphragm, weight eighteen ounces. General enlargement of lymphatic glands all over the body with hæmorrhages into the gland substance. The stomach showed numerous sub-peritoneal hæmorrhages. Bone marrow of the tibia was grey in colour and small in amount.

The outstanding feature of the case was the apparently sudden onset and rapidity of further symptoms.

Case 2.—Sapper V., aged 18, was admitted to the Military Hospital, Fort Pitt, on March 18, 1922. Diagnosis, tonsillitis.

Family History was unimportant.

Condition on Admission.—Both tonsils enlarged and injected. No folliculitis. Temperature 99·2° F. General condition was that of a poorly developed and rather anæmic youth. The throat condition cleared up in three days. It was noticed that the gums were swollen and spongy. The dental officer saw the case and the teeth were scaled and mouth washes prescribed. Bacteriological examination of mouth showed a heavy infection of staphylococci and streptococci. No fusiform bacilli were seen. The cervical glands in the anterior triangle of the neck were enlarged. The mouth condition improved but the gums remained swollen and slight hæmorrhages occurred. The temperature and pulse were normal. The patient ate and slept well, complained of no pain, but showed an increasing anæmic appearance.

A blood examination showed an enormous increase of leucocytes, ninety-eight per cent large lymphocytes. Total count was: red blood corpuscles, 1,000,000; leucocytes, 120,000.

As the case progressed, the cervical, axillary, and inguinal glands became enlarged and palpable. The leucocyte count rose to 180,000 and the anæmia became so marked that the patient became of a "bled white" aspect.

The long bones of the leg and the pelvic bones were radiographed but no change was noticeable in the medullary canals. The gums continued to be spongy, in fact the condition was more marked, but it was merely a clean swelling without sepsis.

The patient gradually sank and death took place on April 20, 1922.

The case differed from the previous case in: (1) Gradual in onset and more prolonged. (2) Hæmorrhage negligible. (3) No purpuric rashes. (4) Temperature remained normal and sub-normal. (5) The marked spongy gum condition. Both young soldiers belonged to the same Battalion and had not served out of England.

No autopsy was possible in Case 2.

I am greatly indebted to Lieutenant-Colonel N. J. C. Rutherford, D.S.O., R.A.M.C., for his help and advice and for permission to publish these notes.

AN UNUSUAL CASE OF OBSTRUCTED LABOUR.

By Major R. KUPER WHITE, D.S.O.,
Royal Army Medical Corps.

Mrs. T., aged 31, second confinement. There had been no difficulty at the first confinement four years previously.

Two years ago she underwent an abdominal operation for a misplaced uterus. There is a small suprapubic scar and subsequent inquiries verified the supposition that there had been a ventrifixation and elicited the fact that there had been stitch abscesses and sinuses persisting for three months.

Labour started with premature rupture of membranes, feeble and infrequent pains commenced some hours later.

In hospital, some seven hours after rupture of membranes, abdominal examination showed a cephalic lie but with the head well above the brim. Vaginally the cervix was high up, near the sacral promontory and pointed backwards; it admitted the tips of two fingers, the anterior and lateral portions were thick and firm, no presenting part could be felt.

Under an anæsthetic, with the hand in the vagina, the posterior lip of the cervix was found to be thin and stretched. The cervix was empty. With fingers in the uterus the head was found to be lying on the lower anterior uterine wall which was thick and firm.

The only effect of uterine contractions was the thinning and stretching of the posterior portion of cervix and lower uterine segment.

An attempt was made to dilate and bring forward the anterior lip of the cervix but this was quite unsuccessful and recourse was had to Cæsarean section.

At the operation the Fallopian tubes joined the uterus about the level of the umbilicus, the lower anterior surface of the uterus was firmly and broadly united to the abdominal wall, about two inches above the pubis, and the uterine muscle itself, especially in the lower part of the incision, was much thicker than usual.

I have no doubt that, as the result of fixation of a large part of the anterior uterine wall, the enlargement of the uterus was almost entirely at the expense of the posterior wall, and that the uterine incision was actually through the fundus, and that the obstruction to labour was the anterior wall lying like a floor across the parturient canal.

Aldershot Military Medical Society.

The first meeting of the Aldershot Military Medical Society since the war was held in the library of the Cambridge Hospital on November 21, when Mr. Maynard Smith read a paper on "Intraperitoneal Abscesses." Major-General Guise Moores was in the chair and there was a fairly large attendance, including Sir Arthur Mayo Robson and many civil practitioners of the district. A short discussion followed. In paying a tribute to the lucid manner in which the lecturer dealt with a difficult subject, Sir Arthur Robson expressed the feelings of an appreciative audience.

INTRAPERITONEAL ABSCESSES.

By S. MAYNARD SMITH, C.B., F.R.C.S.
Late Consulting Surgeon, B.E.F.

Mr. President and Gentlemen,—I must confess that I have had some difficulty in choosing a subject on which to address you to-night. I did not choose a topic connected directly with military surgery because, although no one who worked in France can ever lose interest in that very wide and difficult study, I felt that I should be more at home in speaking on a branch of surgery which forms part of my ordinary daily work. I have selected the subject of intraperitoneal abscesses since it is one which I hope will be of practical interest to all who are engaged in clinical practice, whereas a more specialized topic might appeal to the section only of my audience brought into contact with that special branch.

If you cast your minds back to the days, more or less distant, when you were taught anatomy, you will remember being instructed in the folds of the peritoneum and its relations to and reflections from the various abdominal viscera. You were, however, told little if anything about the potential spaces in the peritoneal cavity—to my mind the most important practical point to the surgeon. Let me explain what I mean by potential spaces. I mean those parts of the peritoneal cavity where fluid extravasations and pus tend to collect, either under the influence of gravity or in obedience to the rule, which obtains elsewhere in the body, of following the paths of least resistance. These spaces are not many in number. Intraperitoneal abscesses practically invariably occur in them and in them only, and a sound knowledge of their position and relations will enable a surgeon to recognize the presence and position of an intraperitoneal abscess and, having done so, to approach it by a safe and direct route and to drain it efficiently.

May I invite your attention to this diagram (I). It shows you a view of the posterior abdominal wall with the viscera removed. You will note the marked eminence running from above downwards in the middle line formed by the marked prominence of the vertebral column —a ridge rendered still more salient by the abdominal aorta which lies on its crest. On either side of this ridge there is a deep valley—deepest of all at its upper end where the curve of the floating ribs forms its floor. This valley, traced downwards, becomes shallower and its bed rises until

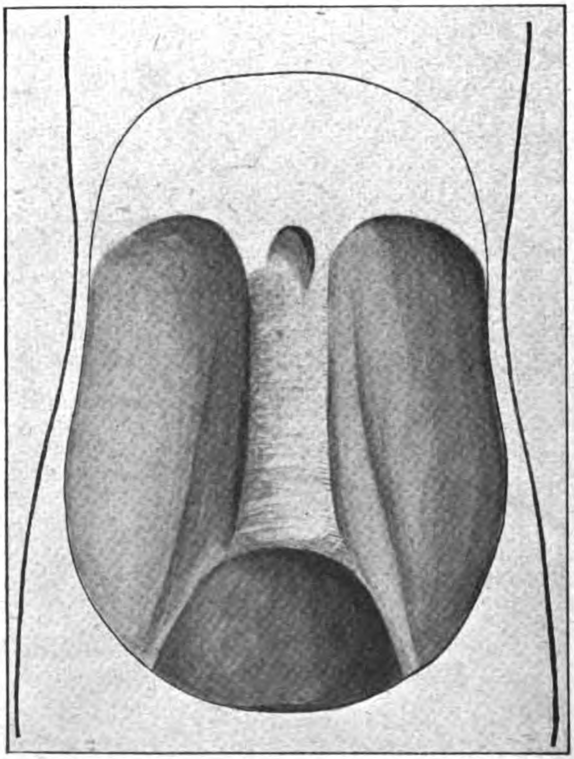

DIAGRAM I.

it reaches the ridge which, passing downwards and outwards on each side, separates it from the deep hollow of the pelvis. These lateral valleys are occupied respectively by the ascending and descending colon. The peritoneum is reflected over the bowel in such a way that a groove is formed on either side of it. To these grooves I have applied the name of the internal and external paracolic grooves of the right side and the left side: the former on each side of the ascending colon, the latter on each side of the descending colon. In the middle line the central ridge approaches the anterior abdominal wall at its lower end and there is a sudden

precipitous descent therefrom into the deep pelvic cavity. If you examine a sagittal section through one of the lateral valleys (Diagram II) you will see how its floor slopes more and more backwards as it descends until its deepest part lies just outside the upper pole of the kidney about the eleventh intercostal space. There is here on the right side one of the most important and capacious potential spaces, commonly called the right kidney pouch of Rutherford Morrison. Let us now turn again for a moment to the diagram of the posterior abdominal wall, and fill in the position of the viscera (Diagram III). We have the two deep lateral gutters occupied by the colon. We have the central part occupied by the coils of small intestine, attached by their mesentery obliquely across the central ridge but so placed that collections of extravasations of fluid may form in the lateral gutters or in the pelvis and yet only affect the small intestines in so far that they adhere and form a roof to an abscess without there being in any way a general peritonitis.

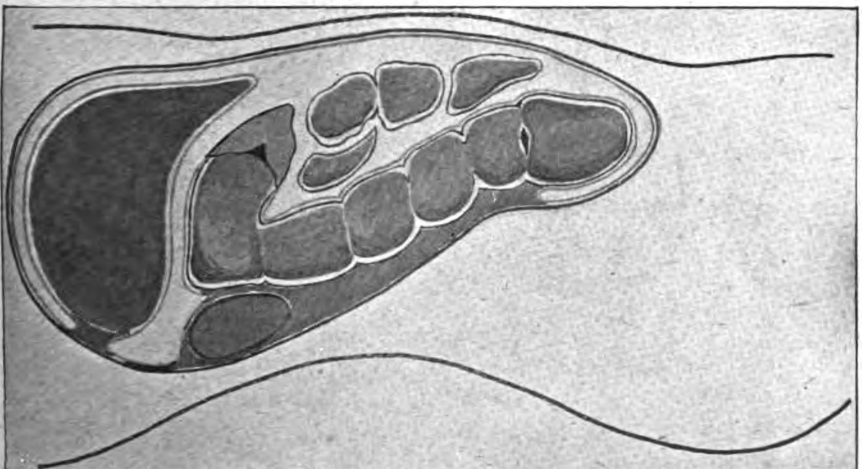

DIAGRAM II.

Many years ago I carried out a long series of experiments on the dead body with a view to ascertaining the course taken by extravasations from perforated duodenal or gastric ulcers and by pus tracking from the appendix. All the information I gained has been confirmed time and again both at operations and at post-mortems on patients dying from these conditions. I, therefore, propose to put briefly before you the nature and result of those experiments. On the dead body a long tube was passed from the mouth into the stomach and the abdomen having been opened an artificial perforation was made in those positions where perforated ulcers are likely to occur. The end of the tube was then brought out from the interior of the stomach or duodenum, as the case might be, and fixed into the

perforation. The viscera were carefully replaced and the abdomen was closed. Some easily recognizable fluid was then run down the tube and the condition of extravasation of fluid from a perforated ulcer was thus reproduced. From perforations of the duodenum and of the stomach in the pyloric neighbourhood fluid ran back into the right renal pouch between the liver and the hepatic flexure of the colon. Here it collected in large amounts : sometimes it ran round the margin of the liver into the subphrenic space. Invariably it then flowed down the outer side of the

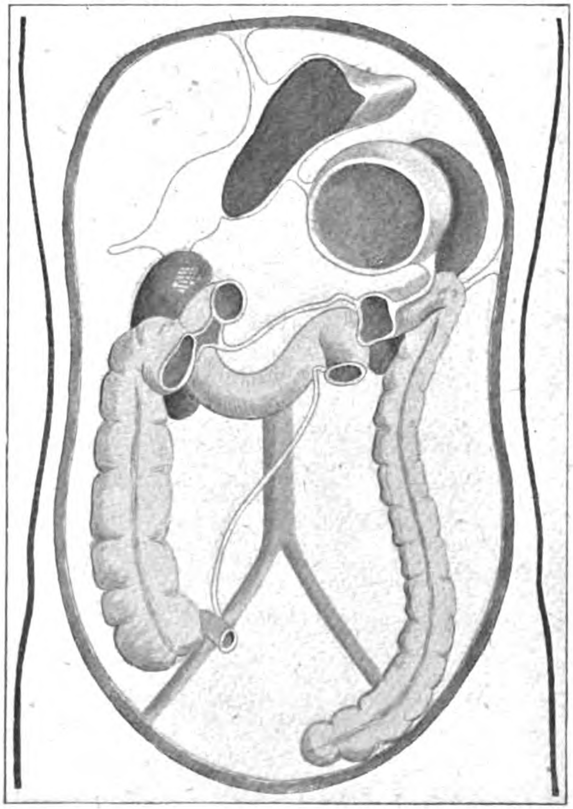

Diagram III.

colon and passed over the brim of the pelvis at the outer side of the cæcum and filled up the pelvis. Occasionally some of it crossed the colon just below the hepatic flexure and passing down the internal paracolic groove reached the pelvis by crossing the ileum. When the pelvis was full the fluid overflowed by crossing the brim of the pelvis on the left side along the inner side of the colon, filling the internal paracolic groove. This space normally is occupied by coils of jejunum and ileum, and permeation of the fluid amongst the coils of small intestine was frequently observed ;

the fluid then crossed the colon near the lower pole of the kidney into the external paracolic groove and so reached the subphrenic space on the left side. In the case of perforations towards the left half of the stomach, fluid ran directly backwards into the left loin and reached the subphrenic spaces direct. Similar experiments allowing fluid to escape from a tube placed in the various positions assumed by the appendix gave the following results. With a tube high up in the external paracolic groove the fluid flowed upwards into the subhepatic pouch and there formed a large collection which occasionally overflowed around the margin of the liver into the right subphrenic space. Where the tube was made to imitate an appendix lying over the brim of the pelvis a large accumulation formed in the pelvic cavity which overflowed on the left side in the manner I have already described. It is in this manner that appendicitis may give rise to a left subphrenic abscess.

In turning now to the practical bearing of these anatomical points I wish to say a word or two first as to the diagnosis of appendicitis. If the appendix lie behind or in close relation to the butt of the cæcum it will be comparatively close to the anterior abdominal wall, since it is in the neighbourhood of that ridge which marks the brim of the pelvis. Tenderness and rigidity will in most cases be typical and diagnosis not difficult. If it lie in the external paracolic groove, particularly when the appendix is a long one and perforated or inflamed at or near its tip, tenderness and rigidity may be more marked in the loin rather than over the front of the abdomen. Even should local signs be obvious anteriorly, the fact that they are situated much above MacBurney's point may lead to an erroneous diagnosis of cholecystitis. Only this week I have had in hospital a case where some three weeks ago the diagnosis of cholecystitis had been made. The condition appearing to subside he had been treated expectantly. A recrudescence of fever, with rigors, tenderness over the liver, and well-marked jaundice, seemed to confirm the original diagnosis. I operated as soon as I saw him and found a long appendix in the paracolic groove, gangrenous at the tip with a small abscess round it. The gall bladder was normal but the liver was swollen, congested, and studded with abscesses—in short a hopeless case of portal pyæmia. On the other hand the appendix may dip over the brim of the pelvis and in proportion to its length become less and less accessible to abdominal palpation. It is this type of appendicitis which I believe is most liable to lead to disaster. The patient, perhaps a child, starts off with acute abdominal pain and vomiting, on palpation there is little or no abdominal rigidity, the hand sinks freely into the right iliac fossa without causing pain or encountering resistance. Deep in the pelvis the appendix perforates: temporarily the child seems better; at any rate, the pain is less; all the time a pelvic abscess is forming, small intestines glue themselves together and form a roof to the abscess, the child's temperature is a little raised but still the abdominal symptoms are masked: pain on micturition when the relief of the tension causes dragging on the inflamed structures

may be the only marked complaint. Then comes the sudden disaster when the abscess bursts, for it is the nature of such an abscess to discharge itself over the brim of the pelvis on the left side and flood the small intestine area giving rise to intense shock and one of the more hopeless forms of general peritonitis. If we consider for a moment the anatomy of the pelvis we shall see that at any stage of the illness a rectal examination would have revealed the condition. Rectal examination alone can ascertain the presence of a peritoneal abscess deep in the pelvis until the abscess has reached such a size that it can be felt projecting above the pubes. Rupture may well take place before this stage has been reached.

Now let us consider an event which presents a not unusual problem after an operation for suppurative appendicitis. The appendix has been removed and an abscess has been drained, and all goes well for a few days. Then the temperature begins to rise again. The patient feels ill once more. The appetite goes and everything points to a re-collection of pus somewhere. There are four definite sites where this collection is most likely to be found :—

(1) Pain, tenderness and swelling in the neighbourhood of the wound will suggest that the collection is in the region of the cæcum. Such a collection may readily be drained by introducing the finger along the track of the original operation.

(2) Should there be no signs in the right iliac fossa, the rectum should be examined to ascertain whether there be an abscess forming in the pelvis. At times such an abscess will give rise to no symptoms recognizable by abdominal palpation. At other times a zone of tenderness and rigidity in the suprapubic region will indicate plastic peritonitis where the intestines are glueing themselves together, forming a roof to the abscess. Not infrequently, accompanying a pelvic abscess, there is a tender swelling in the neighbourhood of the brim of the pelvis on the left side. It is formed in part by the matting together of the sigmoid and small intestine in the region which I have already described as being the path by which collections of fluid are prone to make their way out of the pelvis. In part, also, the swelling is due to the holding up of the contents of the sigmoid near the pelvic brim.

A pelvic abscess may usually be reached by passing the finger from the original wound over the brim of the pelvis on the right side, keeping close to the posterior abdominal wall and being careful not to break down adhesions anteriorly. Failing this a middle line incision may be made and the finger passed down into the pelvis, keeping close behind the pubes. In either case the finger of the other hand in the rectum may act as a useful guide. The greatest care is necessary as it is very easy to damage the bowel and cause a fæcal fistula.

(3) Tenderness, pain and rigidity beneath the costal margin on the right side, outside the edge of the rectus and reaching back towards the right loin, will suggest the presence of a subhepatic abscess, that is to say

an abscess in the right kidney pouch. With one hand posteriorly and another anteriorly (as placed in examining for a kidney enlargement) a definite mass will sometimes be felt. Occasionally such an abscess may be accompanied by some dullness and loss of air entry at the base of the right lung. This type of abscess is best reached by an oblique or transverse incision below the ribs reaching towards the edge of the rectus. A pack is placed anteriorly to prevent the first gush of pus when liberated from spreading infection to the general peritoneal cavity. The finger is then passed backwards and inwards beneath the liver. Adhesions will be usually found between this and the hepatic flexure of the colon, and on gently breaking these down the finger will enter an abscess cavity in front of the peritoneum covering the right kidney.

An abscess of this nature may pass upwards a short distance behind the liver around the right lateral ligament; the term right posterior subphrenic is sometimes given to this extension.

(4) The term right subphrenic abscess (sometimes called the right anterior subphrenic) is applied to a collection of pus bounded above by the diaphragm; below by the extensive convex surface of the liver; to the left side by the falciform ligament; posteriorly by the coronary and right lateral ligaments; anteriorly and externally by adhesions between the margin of the liver and the abdominal wall. The presence of a right subphrenic abscess may be suspected in the first place if the train of symptoms already described occur after acute appendicitis, and careful examination does not reveal the presence of pus in any one of the other three situations already dealt with. The signs at first will be mainly thoracic; there will be dullness, loss of air entry and vocal resonance at the right base behind and laterally. The dullness does not show the typical curve of a pleural effusion. The heart is, as a rule, not obviously displaced. There is an absence of adventitious sounds and no pneumonic expectoration. Careful palpation will soon reveal a zone of tenderness along the costal margin, often well marked in the angle to the right of the middle line. In this stage most valuable evidence can be given by an X-ray photograph which will show the dome of the diaphragm pushed up two or three spaces above its normal level and its movements with inspiration absent, or greatly restricted. Should gas also be present in the abscess cavity, the X-ray picture is pathognomonic. Of course if the subphrenic abscess has been present for some time a pleural effusion, at first serous and afterwards purulent, may result. A subphrenic abscess is very commonly mistaken for an empyema and, as I have just shown, may be accompanied by it. In an empyema a needle reaches pus as soon as it has penetrated the chest wall. In a subphrenic abscess a needle reaches pus at a depth of 2 in. or more, and sometimes its movements with respiration will suggest that the needle has pierced the diaphragm.

An abscess of the type I have been describing may follow also perforated gastric or duodenal ulcer. The symptoms of the established abscess will

be identical with those already detailed. While it is possible to reach a subphrenic abscess by cutting down along the costal margin and separating the liver from the parietes, it has been my constant experience that such anterior drainage is ineffective. I invariably drain these abscesses by removing a piece of the ninth or tenth rib a little behind the posterior axillary line. The diaphragm has been forced up by the abscess—one is below the level of the lung. The two layers of pleura, diaphragmatic and parietal, may be firmly adherent. Should this not be the case, a continuous suture of catgut attaches the opening already made in the parietal pleura to that covering the diaphragm. An oval pleura-covered area of diaphragm is thus brought up to the bottom of the wound and the pleural cavity completely shut off. The diaphragm can then be cut through and the abscess reached. I have never infected the pleura in treating subphrenic abscesses in this manner, whilst ideal drainage is obtained. Supposing that when one opens the pleural cavity one finds that there is already an empyema, I should then confirm my diagnosis of a subphrenic abscess by passing a needle through the diaphragm held steady with forceps for the purpose. If this confirmation were obtained, I should then bring the diaphragm up to the chest wall, cut through it, and drain the subphrenic abscess exactly as already described, and drain the empyema separately by removing a higher rib further forward.

It will be convenient here to say a word about left subphrenic abscesses. They are rare following appendicitis; they are commoner following perforation of a gastric ulcer, especially if it be some distance to the left of the pylorus.

Generally speaking, the symptoms are similar to those of a right subphrenic; their anatomical relations are less constant. In many cases it will be found best to make an incision along the left costal margin. After packing off the peritoneal cavity below and towards the middle line the fingers are passed upwards under the diaphragm, and adhesions gently separated, until the abscess cavity is reached. Should the cavity be found to extend high up under the dome of the diaphragm, it is best to make a counter-opening by removal of a rib in a manner similar to that already described.

I trust that I need make no excuse for dealing in some detail with subphrenic abscesses. The diagnosis of abscesses there is not infrequently missed. Unfortunately the elaborate (though perfectly correct) classification of all sorts of varieties of these abscesses has tended to make the subject hopelessly obscure. I have tried to describe them as simply and practically as possible, and I thank you for the patience with which you have listened.

I cannot terminate without expressing to you the great pleasure it has been to me to address to-day members of the service with which I had the honour to serve for five years—in South Africa in 1900, and in France more recently, and more especially to do so under the presidency of my old friend and indulgent chief, General Guise Moores.

Echoes of the Past.

THE ORGANIZATION OF MEDICAL AID IN THE FIRING LINE.[1]

BY S. F. CLARK.
Royal Army Medical Corps.

THE whole subject of army medical matters is a large one, and, therefore, it is possible for me to-night to touch upon only one aspect of it, viz., the organization of medical aid in the firing line. I shall interpret the words "firing line" fairly liberally, but, at the same time, shall not take my hearers out of the sound of the guns. It is not easy to make such a lecture as this interesting to non-medical men, but I trust that those officers here who belong to other branches of the Service may learn something of our methods in war, and that an interest in our work may be initiated or amplified.

In savage warfare the treatment of the wounded is drastic and effectual so much so that soon after the action there are no wounded, and the victorious army moves off unhampered; but the advance of civilization has discountenanced these strong methods, so we have the incongruity of the highly civilized nations equipping one body of men to destroy human life, and another to save it. I shall endeavour to put before you to-night the methods in use in our own Army for succouring the wounded, and you will probably be surprised to hear that a real attempt to help effectively our soldiers injured in battle is of comparatively recent date.

The history of the medical arrangements of our Army shows that for centuries the wounded were not greatly considered, and primitive and utterly inadequate was the organization for their help. Military surgeons were present at Cressy—five and a half centuries ago—but the centuries rolled on, while medical arrangements stood still, and while our long and glorious roll of victories was being compiled, those soldiers who fell wounded in them were in evil case—for their sole help remained as it had been at Cressy. There were surgeons present but little else, no assistants, no stretcher-bearers, ambulances, or proper dressings and equipment, and the surgeons were, therefore, apparently expected to cure the wounded by some sleight-of-hand performance, or by some mysterious healing virtue that emanated from them as medical men. The result, of course, was that, what to us is a comparatively trifling injury, in those days meant the death of the patient, and the awful suffering which must have followed the incessant fighting in Europe is too terrible to dwell upon. Even in the Peninsula and Waterloo days, practically nothing was done for the wounded until the troops had time to bring them in—generally the next

[1] Lecture delivered on January 24, 1902, before the United Service Institution, Hong-Kong.

day—and from lack of proper transport and other necessaries it was hopeless to give them proper attention. In the long peace from 1815 to 1854, practically nothing was done in the way of reform either in the medical service or in the army generally, and the service was, in every sense of the word, drifting into inefficiency and ignorance of war routine, and the Crimean campaign came like a thunderbolt on an army in every way unready for war. A great awakening came on the nation and reacted on the medical service. When the British Army embarked for the Crimea, it may safely be said that no army medical service existed. The battalions had medical officers, and a serjeant and a few privates were told off for duty in the regimental hospital, but there was no medical service in the sense it is now understood in modern armies. The regimental system prevailed by which each battalion had two, and in India three or four, medical officers, and a regimental hospital attended to and nursed by regimental orderlies. These officers belonged to the battalion just as the other officers did—they were gazetted to it, wore its uniform, and spent their lives in it. They had no command power and no real responsibility, as the regimental commander was responsible for the discipline and the efficiency of the hospital. Under this system the medical officers were solely physicians and surgeons of the hospital, and the military commander undertook all the rest of the work. This may have been all very well in peace, but when war broke out, and an action had been fought, the weakness of this system was exposed. The one tent which represented the regimental hospital was hopelessly inadequate for the number of wounded; attendants were too few, appliances and dressings were wanting, there were no special men to remove or collect the wounded, and if the battalion pressed on, the surgeon and his few assistants were left on the field to deal with the casualties as best they might. There was no organization to take the patients back to the base, no hospital at the base to receive them even if they got there, and altogether everything was chaos and confusion. When the hospital at Scutari was formed the medical officers who had been trained merely to prescribe for the sick were unable to manage or organize a large war hospital. As Miss Nightingale said: "No one seemed able to make the machine go"—the certain sequence of inefficient peace training in war routine.

After this war the Commission, which now seems to come regularly after each campaign, was held, and as a result certain reforms took place, but the regimental system remained untouched until 1873, when the German campaigns of 1866 and 1870 had given a fresh impulse to military matters, with the result that the medical regimental organization was done away with, and a Medical Corps for the Army developed. This was done by detaching the medical officers, the hospital serjeants and the nursing orderlies from the battalions, and grouping them into the elements of a Medical Corps, to which, in 1877, Mr. Gaythorne Hardy gave a military command over itself and its members, and the autonomous life of the

Medical Corps began. The bearer company and the field hospital, the hospitals on the line of communications, and the general hospitals at the base began slowly to develop, and a new responsibility, heretofore not given to the medical service, was now thrown upon it. A system of medical field organization, copied exactly from Germany, France, and the modern armies of every State, grew up, and by it the sick or wounded soldier is completely withdrawn from the fighting front of the Army, and from the day he is struck down by sickness or the bullet he sees his battalion or battery no more unless he recovers and rejoins it, but if he is a serious case he passes, still under the detached and independent army medical care, to the base of operation and thence to England. At any rate his own commanding officer or his comrades have no responsibility for his cure or carriage, and he remains absolutely and entirely in the hands of the medical service. A corps becoming thus executive in its functions and relieving the military side of what was formerly its heavy executive task, gradually worked into an actual corps, and in 1885 the officers became the Army Medical Staff and the men, who had been known as the Army Hospital Corps, became the Medical Staff Corps. These two separate bodies were in 1898 welded together as the Royal Army Medical Corps. This evolution was gradual but certain, and all medical officers of any seniority will remember the protracted conflict between the War Office and the medical profession, the stubborness with which both sides fought, and the series of victories which the profession gained. So long as the medical service of the Army remained a regimental organization, with the regimental commander responsible for all discipline, all order, and in the end all efficiency in the military hospitals, the medical service made no claim for military titles or command, but served tranquilly under the battalion military commander, who, being responsible, relieved them of all anxiety for the sick and wounded—at any rate in theory. But when modern war requires that the field army should be no longer clogged in its movements by sick and wounded, and that all these impediments should be taken over by some new responsible body, the medical service became charged with the entirely new responsibility. With the sick massed in the field hospital of war, the medical officer, far away from the help of his colonel, felt the need of military authority to keep order and ensure obedience, and so along the whole line of communications from the battlefield in front to the base and England, the medical service was militarized to give it the power and the authority to carry out its new responsibility. This was the logical outcome of the movement of 1873 towards medical responsibility, and it was impossible to make the Medical Corps an executive corps within itself and at the same time withhold the military titles. Thus we see that, after our centuries of warfare, it is only within the lifetime of, I suppose, most of us in this room that a real attempt has been made effectively to succour the wounded; and the weak, inefficient and crippled medical service of little more than twenty years ago, is now a vigorous Royal Corps, which, in spite of all that its detractors

may say, has done magnificent work in our recent fighting. Any apparent failure has been mainly due to the fact that one man cannot do the work of four, and that a hospital equipped for 100 beds cannot just as easily accommodate and provide for 400 men.

After this brief history, let me now approach more immediately to the subject of this lecture. The whole aim of present-day organization is to provide in war time first-aid on the field, to rapidly remove the wounded from the field to the elementary shelter in the field hospitals, to transmit them towards the base through fairly comfortable stationary hospitals on the lines of communication, to care for them in the great base general hospitals, to transfer them to England in hospital ships, and thence to the great military hospitals at home. The Medical Corps does all this, and so it not only treats the wounded but it clears the front of every ineffective man and so keeps the field army efficient and mobile. What a contrast this is to the battle of the Alma, where it is said that the army might have entered Sebastopol immediately after the action only that it had to remain to assist with the wounded. If this is true it emphasizes the enormous military importance of an efficient medical organization.

Some writers hold that in modern war the idea of instant succour to the wounded is a mistake, on the ground that it will cause so great a crippling of the Medical Corps during the action by their exposure to fire that after the action the number of skilled men available to look after the masses of wounded will be inadequate, and that, in consequence, the wounded will be worse off in the end. This is a legitimate argument, and though it is not yet acted upon, yet there is no doubt that, if the casualties among the officers and men of the Medical Corps become disproportionately great, it will have to be acted upon, for in modern war the men moving about in the open attending to the wounded will get more than their fair share of fire, and will be unable to use cover to any extent. Even against the Boers, wounded had sometimes to be left where they fell, notably in the Natal fighting, simply because to attempt to reach them would have been tantamount to suicide, and in these days of long-range weapons a Red Cross armlet is no protection to a man.

I cannot do better than follow the lines of the traditional sermon, and divide my subjects into three heads—firstly, secondly, thirdly and lastly.

Firstly.—What can the wounded man do for himself?
Secondly.—What can his own corps do for him?
Thirdly.—What can the R.A.M.C. do for him?

(1) *What can the wounded man do for himself?*

In our Army every officer and man has a first field dressing served out to him when he goes on active service. It is sewn into a special pocket of his coat and he is taught the use of it. The intention is that as soon as he is struck, the man himself or one of his comrades may at once apply an antiseptic dressing to the wound, and also have a means of, to some extent,

controlling hæmorrhage. On my way to North China in 1900, every man on board was instructed, not only in the use of these dressings, but also in first-aid to the injured, and it was rather a coincidence that one of the chief prize winners in an examination that we held at the end of the voyage was killed in his first action. The dressing is of great value, as it gives the wound a chance from the first by bringing antiseptics to bear upon it at once, and prevents dust and dirt of all descriptions from getting in and poisoning the wound. At the same time it is quite possible that we expect too much of this dressing, for it is one of those cases in which what looks very nice in theory does not always come up to expectations in practice. Let us look at the conditions under which a man is wounded in the field. He is probably lying on the ground behind a stone or some other slight shelter, and all round him is a shower of bullets which renders it quite impossible for anyone to walk up and attend to his wound properly, or to carry him away at once. He must lie where he is till the action is over, or, at any rate, till it has passed away from his vicinity, and until that happens the only assistance he can get will be either by his own efforts or from a comrade, or from the surgeon attached to the battalion, who makes his way to him as best he can, and naturally as unencumbered with apparatus and paraphernalia as possible. Under these circumstances, what happens to the wound? The South Africa experience has shown that if there is much hæmorrhage it very soon soaks through the small amount of material which comprises the first field dressing, and in consequence of this soakage and also owing to the fact that the dressing contains only a small amount of antiseptic material, putrefaction in the wound is not long delayed. The dressing which the soldier carries with him, therefore, is more a sort of clean rag than an actively antiseptic dressing. But under the circumstances, is it even a clean rag? The soldier or his comrade opens the packet, their hands are covered with mud and sand and grease, they take out the dressing with their dirty hands and probably place it on the ground while they are getting the clothes opened, and then they stick the dirty dressing on the wound. Or, suppose the surgeon puts it on, are things much better? He cannot carry bottles of antiseptic lotions with him into the firing line, he cannot even wash his hands, and though he probably will not lay the dressing on the ground, yet even with the greatest care the dressing will not be an antiseptic one in the true sense of the word. Thus we see that, valuable as this first field dressing is, yet we must not expect too much from it, and it is quite possible that, after this war, the contents of the package may be modified; the introduction of a small quantity of a fairly strong antiseptic powder into it would probably be an improvement.

Again, if the wounded man has some technical knowledge he can still do more for himself. By keeping quiet he can prevent a simple fracture from becoming the more formidable compound fracture, or he may even practically save his own life as was done by Captain Dalton, R.A.M.C., who

was shot through the abdomen in one of the South Africa actions. He lay where he fell for twenty hours, I am told, eating nothing, and drinking nothing, and allowing nobody to move him. By these means he gave himself every chance and eventually made a good recovery. Had somebody in mistaken zeal endeavoured to win a V.C. by throwing him across a horse and carrying him off from a civilized enemy, there would have been one grave more in South Africa. It would be interesting to know the after-history of wounded men carried violently away from an enemy who would do them no further hurt ; it would probably be found that many a cross had cost the life of the so-called rescued man. Of course I make no reference to savage warfare, which is quite a different matter.

(2) *What can the wounded man's corps do for him?*

Formerly, a man had only his own corps to look to, for the simple reason that no Medical Corps existed. His comrades carried him away when he fell, but as even our own army has its share of skulkers and men with no stomach for fighting, it too often happened that these comrades forgot to return to their places, and, in consequence, the effective force of the corps was much lessened. I have read that, in the Crimea, a soldier wounded in the hand and quite able to walk would be accompanied back by four or five anxious comrades all full of sympathy—who were seen no more in the fight. Most of us have heard how Sir Colin Campbell gave orders that the wounded were to be left where they fell, and that if any man left the ranks to carry them away, his name would be posted on the door of his parish kirk. This stopped the practice in Sir Colin's brigade, for no man wished his kinsmen at home to think that he had brought discredit on the Highland name by cowardice in battle. But we have changed all this haphazard work, and now each corps has its own trained stretcher-bearers—two men per company—who take no part in the actual fighting but attend to the wounded of their own battalion.

In peace time these men are regularly trained by the R.A.M.C. in stretcher drill and first-aid, and they are prevented from becoming rusty by a monthly drill. In this station this drill is held on the first Tuesday in each month. It must be understood that these are not R.A.M.C. men at all, but are soldiers of the fighting unit—red-coated men. When an action is expected, they are placed under the orders of the medical officer attached to the unit, and attend to their own wounded. When the action is over they return to the ranks of their unit, and the medical officer does not see them again until the next fight is expected. Although the regimental system no longer prevails in time of peace, yet in war a medical officer is attached to each battalion, cavalry regiment and artillery brigade, and as he is for the time being practically an officer of the unit, it will be convenient to include him in the assistance rendered by the corps itself. Let us then turn our attention for the moment to the regimental medical officer and his assistants, and see how they work. He is under the orders of the officer commanding the unit to which he is attached, but also receives

instructions from the principal medical officer of the division in which he is serving, and is at the disposal of that officer. His unit carries his baggage and provides him with a servant, and in addition provides him with one lance corporal and also with one private as an orderly. This man also drives the cart in which the stretchers and equipment are carried. This equipment consists in an infantry battalion of 1 medical field companion, 1 surgical haversack, and 1 pair of field medical panniers. Their total weight is about 183 lb., and, as I have said, they are carried in the regimental medical cart, and the commanding officer arranges that the cart is at all times at the disposal of the medical officer. When an action is expected the trained stretcher-bearers report themselves to the medical officer, place their rifles and valises in the cart, put on the stretcher-bearer's armlets, take the stretchers and proceed under the medical officer's directions to the scene of action. The orderly accompanies the officer, carrying the field companion, water bottle, and surgical haversack, while the lance-corporal remains in charge of the panniers so that they may be made available during or immediately after the action. This medical officer and his men must never lose touch with their corps during the fighting, but must keep in close proximity to it, and on no account attempt to carry the wounded back for long distances or in any case beyond the collecting station formed by the bearer company. The duty of this party is to render temporary aid only until the wounded are succoured by the bearer company. The regimental bearers are not supplied with any materials for dressing wounds, but must use the field dressings of the wounded. The medical officer applies any further dressings that he may deem necessary, arrests bleeding, applies splints, combats shock, and so on, but he will not undertake any serious operation. To each wounded man he affixes a tally, stating the man's corps, number, rank, and name, together with the nature of the injury, the treatment adopted, and any precautions necessary in moving the man. This is for the information of the bearer company and field hospital. Green coloured tallies are placed on serious cases requiring immediate attention. As the unit advances so does the medical officer and his men, and they are directed to collect the wounded in groups as far as possible so as to facilitate the work of the bearer company, which does the actual carrying of them off the field. In civilized warfare the work of this regimental party is comparatively plain sailing, especially in a victorious advance, but if the conditions are reversed and the unit is rapidly retreating before the close attack of a savage foe, the position of this party is by no means enviable, for they must be among the last in the retreat, and the risks they run are proportionately increased. Our organization, however, is based upon civilized warfare, so it is evident that this regimental aid is intended to be of an emergent and temporary nature—and, practically, is the connecting link between the wounded man and his more elaborate and systematic treatment by the R.A.M.C. Observe that this party makes no attempt to remove wounded from the field—the bearers bring the wounded to the regimental medical officer or place them under shelter, if possible,

while the medical officer merely does what is necessary to save life at the moment, and then turns his attention to the next man. They leave the wounded lying on the ground while the tide of war carries them on with their unit.

It must be evident to you that if the action is one of any severity and casualties are numerous that the aid given in the front line is quite inadequate for the requirements of the situation. The battalion is possibly 1,000 strong and the single medical officer has not one man of the medical corps to help him. His assistant is a lance-corporal of the battalion, a raw man who never saw a wound or assisted at an accident in his life, and simply is given the billet for the campaign. Very likely too a different man frequently reports himself to the medical officer stating that the other man has been assigned a fresh duty. The orderly is no better, and the sixteen stretcher-bearers have after all had only an imperfect and elementary training, and though they carry four stretchers between them and each stretcher acts more or less independently, yet there is not even a surgical haversack supplied for each stretcher. If the medical officer gives the bearers any bandages, etc., out of his small stock he very soon finds himself without any. And think of the extent of ground that is covered by 1,000 men in a modern battle—probably the front extends for several hundred yards—and to this are assigned four stretchers with practically no dressings, and one medical officer without a single skilled man to help him. Every other army in Europe of any importance puts two medical officers to each battalion and attaches at least four men of the medical corps to it, as assistants to the officers. They also have a senior medical officer for each three battalions who supervises the work of the battalion medical officers. Thus each 3 battalions have 7 medical officers and some 12 orderlies for skilled aid in the firing line, while in our service there would be 3 medical officers and no skilled orderlies, and no supervision from a senior officer. We see thus that there is a serious shorthandedness in the very first line, and the wear and tear on the medical officer must be excessive. This shorthandedness clings to the R.A.M.C. all over the world—in peace and in war—and I believe the Naval Medical Service is beginning to feel the same thing.

Now we reach thirdly and lastly, viz..—

What can the R.A.M.C. do for the Wounded Man?

This brings us to a consideration of the first stages in the long line of medical aid between the battlefield and England. The general plan is this: the bearer company carries the man first to the collecting station, either on a stretcher or a hand seat. Thence he is conveyed in an ambulance wagon to the dressing station, and afterwards in another wagon to the field hospital. After a short sojourn here he starts for the base—if possible in a hospital train—and he breaks the journey in the various stationary hospitals established on the lines of communication. He eventually reaches the general hospital at the base, and if he is not likely

to be soon fit for service, he is sent to England in a hospital ship, and on arrival goes to Netley or other large military hospital for final disposal. The whole of these arrangements are made by the R.A.M.C. so that it can easily be seen how in South Africa there were constantly thousands of men in their charge. I again ask the military officers, and naval officers too, if they think a vast organization like this could be successfully run by officers without the authority of definite military titles and rank? I think not.

The first link in this R.A.M.C. chain between the firing line and England is the bearer company. This gathers in all the wounded of every branch of the service on the battlefield, and frees the battalion altogether from any responsibility for their wounded, who, from this unit onwards towards England, see their battalions no more during the campaign. One would imagine that so needful a unit as this would be very complete, but it is quite the reverse. The company is a purely scratch formation, it exists nowhere in peace and is thrown together only for the war. Officers and men may never have seen each other before, and it misses, therefore, the cohesion and solidarity of more definite organization. These companies were first founded in 1878, and originally each company had one medical officer, and nine men more than to-day, when firearms are far more deadly and casualties liable to occur more quickly than formerly. There was also one quartermaster to each two companies, but he has since been abolished —thus throwing much non-professional work on the commanding officer. A bearer company of to-day consists of 3 officers, 1 warrant officer and 57 non-commissioned officers and men. These sixty-one individuals are collected from all over the British Islands and rendezvous at Aldershot, where they become bearer company of such-and-such a number and the commanding officer has to weld this "fortuitous concourse of atoms" into an organized body. In addition to his own 60 R.A.M.C. officers and men, he has attached to him and under his command 1 warrant officer and 37 non-commissioned officers and men of the A.S.C. for transport duties—so that the company all told consists of 3 officers, 2 warrant officers and 94 non-commissioned officers and men. A bearer company is formed for each brigade of cavalry and infantry and after the commanding officer has drawn his equipment and has satisfied himself that his company is complete in every respect, it proceeds to the place of embarkation. The regulations lay down that the whole company with its material and transport is to embark at the same time as the brigade to which it is attached. The company is officially attached to a certain brigade, but it is at the disposal of the general officer commanding the division, who can detach the companies or mass them as necessity may require. During the voyage the major R.A.M.C. in command learns something of his two junior officers—who may be either captains or lieutenants—he studies his non-commissioned officers and men and instructs everybody as far as possible in their duties, so that by the time the seat of war is reached all ranks are bound together by the magic of *esprit de corps*. I must emphasize the fact that

in peace time not even the nucleus of a bearer company exists and that even in manœuvres a company is rarely, if ever, mobilized, so that in war the company has to perform duties which it has never practised in peace and it makes its first appearance as a going concern on the battlefield of real war. How would my combatant bearers like to conduct a real attack with troops who had never practised it before on the drill ground? Among other weak points is the fact that the company cannot sub-divide into sections to work with each of the battalions of the brigade if separated or detached, and the whole of the equipment and instruments ought to be increased on a liberal scale so as to admit of decentralization into groups to serve with battalions on detached expeditions or movements. The ambulance wagons too belong to the A.S.C. in peace, but to the R.A.M.C. in war and though the discomfort and unwieldiness of these wagons has been protested against, yet at the end of the operations they are returned to the A.S.C. and the Medical Service has no further voice in the matter until the next campaign brings out the unchanged ambulance wagon. The bearer companies are too few in number for effective work, each individual company is deficient in personnel and is weakly equipped. The bearer company for a cavalry brigade also is exactly the same as for an infantry brigade—no attempt is made to give it extra mobility. In addition to these hindrances to effective work, the last straw was the regulation made in 1887, or 1888, that recruits for the R.A.M.C. were not to exceed 5 feet 5 inches in height. I ask you how far can an undersized man of 5 feet 3 inches to 5 feet 5 inches be expected to carry a well-developed dragoon or seasoned infantry man? This sort of thing disheartens the Medical Corps—being given the smallest men to do heavy manual duty. The transport of the bearer company consists of 10 ambulance wagons, 4 forage carts and 1 water cart, and in addition to the horses for the 3 officers there are 53 public horses supplied—4 being allowed for each ambulance wagon.

On the battlefield the duty of the bearer company is to convey the wounded from the firing line to the field hospital. There its duty ends; so that, except when actual fighting is going on, the company is, theoretically at least, unemployed. But to prevent this waste of strength bearer companies are directed to be associated with the field hospitals and encamped near them, in order that they may assist these hospitals in every way in their power, but the organization of the company is kept separate and distinct from the field hospitals.

Let us now imagine the company in action and see how it is distributed between the firing line and the field hospitals. The R.A.M.C. contingent is thus disposed of: 2 stretcher sections are sent out to succour and collect the wounded—they absorb 1 officer and 34 non-commissioned officers and men. Their "beat," as we might call it, extends from the firing line to the collecting station. At the collecting station are one serjeant and the bugler. At the dressing station is the major in command, the remaining junior officer, the serjeant-major and seven non-com-

missioned officers and men—one of whom is a cook. Ten non-commissioned officers and men are employed with the ambulance wagons and the remainder—four in all—stay with the baggage and prepare food for the company. Seven of the A.S.C. section are with this party, while the remainder are with their respective vehicles. The ambulances are divided into two lines—the first travels between the collecting and the dressing stations and the second between the latter point and the field hospital. The water cart and equipment carts are at the dressing station.

In the absence of any instructions from superior authority the officer commanding the company makes all these arrangements himself. He decides what proportion of his wagons are to be in each of the first and second lines—this depending mainly on the character of the enemy, the rapidity with which the wounded are being brought in, the state of the roads and the distance between the collecting and dressing stations and the dressing station and field hospital. He also chooses the positions of the collecting and dressing stations—the general principle being that the collecting station must be as near the fighting line as possible and must be at no great distance from the dressing station—so as to shorten the journeys of the bearers and ambulances of the first line and bring the wounded within reach of surgical aid as speedily as possible. In some cases—and invariably with mountain equipment—these two stations are combined. Collecting and dressing stations, moreover, must advance and retire on a forward or a retrograde movement of the troops being made. All these stations are marked by red cross flags and similar flags are placed between the two stations and also between the dressing station and the field hospital so as to mark the road for the waggons and as a guide to any wounded who may be able to walk—such as men struck in the arms.

Let us now take each of these portions of the company in detail. We will begin with the two stretcher sections which search for the wounded. To each stretcher four men are told off—two actually carry the stretcher with the patient on it, while the others act as reliefs and also take back the wounded man's arms and equipment—which accompany him back to the field hospital and are there handed over to the Quartermaster. One of these four bearers acts as commander of the squad—as the four of them are called—and he carries a surgical haversack and water bottle. While the two actual bearers are opening out the stretcher and preparing it for the patient, the other two attend to his injuries and then all four lift him on to the stretcher. Each stretcher squad acts independently of the others, but to each four stretchers, constituting a section, is one non-commissioned officer who controls his section, while the officer supervises both sections. So that after all the bearer company sends out only 8 stretchers, with 32 privates as carriers and 1 officer and 2 non-commissioned officers as supervisors. The officer, of course, does what he can professionally for each patient, but the total aid seems very inadequate for a brigade of four battalions on a war footing. These stretcher bearers also pick up the wounded which have been attended to by the regimental medical officer

and his men in the first place and after rendering immediate aid to all wounded that they find, they carry them to the collecting station, place them, stretcher and all, in the wagons, take fresh stretchers and return at once to the scene of action. They will on no account pass in rear of the collecting station.

The collecting station is marked merely by a red cross flag stuck in the ground and one serjeant and the bugler constitute its staff. It ought usually to be under shelter from fire, if possible near a road, but as near the fighting line as is consistent with safety. It is merely a depot for transferring the wounded from the bearers to the ambulance wagons. The wagons of the first line rendezvous here and as fast as each is loaded it starts for the dressing station. Each can take only two lying down cases and the R.A.M.C. non-commissioned officer, or man, told off to each wagon is responsible for them on the journey down the road. After taking the wounded to the dressing station these wagons return at once to the collecting station and they never go in rear of the dressing station until the field has been cleared. The serjeant in charge of the collecting station has a field companion and water bottle in his care and also a small reserve of bandages and first dressings to replenish the surgical haversacks of the bearers.

The next place is the dressing station—it is more elaborate than the collecting station, and is, if possible, out of fire near a road, while advantage should always be taken of a good water supply and of buildings, or other shelter, in the vicinity of the scene of action. If no building is available the operating tent is pitched, and the medical and surgical equipment, medical comforts, water cart, and second line of ambulance carts are here assembled. The cook lights a fire and prepares beef-tea, and stimulants are also got ready. Two officers, the serjeant-major, and seven non-commissioned officers and men take up their position here, and more can be done for the wounded than is possible nearer the actual fighting, but at the same time only urgent matters can be attended to, and no operation is performed unless it must be done at once to save the patient's life. Here the original dressings are examined and re-arranged or amplified if necessary, splints are adjusted with greater care than was possible amid the whistling of the bullets, food and drink are given, and the patient prepared as far as circumstances permit for the next stage of his journey. As the examination and dressing of each man is completed he is placed in a wagon of the second line, and conveyed to the field hospital. As soon as the men have been handed over to the field hospital, the wagons return to the dressing station. This process of passing on the wounded from group to group of the bearer company goes on as long as the fight lasts, and when all the wounded have been removed from the open, the woods and ditches in the neighbourhood are methodically searched so that there may be no possibility of any wounded remaining uncared for. Lanterns for searching in the dark form part of the equipment of a bearer company. If the regimental medical officer has not already fixed a tally on any wounded,

the officers at the dressing station affix one—green or white as the case may be—for the information of the field hospital. When the whole of the wounded have been transferred to the field hospital, at the close of the action, the bearer company rendezvous at the dressing station, and then join their brigade in the vicinity of the field hospital.

This is the typical arrangement of a bearer company, but it cannot always be followed. In mountain warfare, for instance, where vehicles cannot go, the wounded are placed on cacolets or folding chairs, two to each animal, and are thus carried by pack transport. One cacolet is slung to each side of the pack saddle, and horses, mules or ponies are the animals employed. The drawback to this is that the patient has to be in a sitting position, which is very unsuitable for many forms of injury. Formerly litters were supplied on which patients could be carried by pack animals in a lying down position, but they were abolished some years ago, for what reason I do not know. All stores and equipment of course must be carried by pack animals in mountain warfare, and in such conditions the collecting and dressing stations are combined.

Some of you may know that in the late fighting in Natal, a corps of civilian stretcher-bearers was formed, and they used to carry the wounded great distances—the wheeled transport being largely discarded. These men carried the patients from Intombi Camp, twenty-six miles, but there were eight men to each stretcher and some 2,000 bearers in all. The undermanned R.A.M.C. could not do such things without civilian aid.

In India again, where the non-commissioned officers and men of the R.A.M.C. do not serve, the bearer company is unknown. Its place is taken by a cloud of dhooly bearers with their clumsy dhoolies, but the way in which these weak looking men will go on mile after mile with their burden, and will take it over apparently inaccessible places excites the wonder and admiration of European beholders. They show courage too in the firing line. The regimental stretcher-bearers exist as I have described them—in both European and native troops.

In the Egyytian campaigns where our troops so often fought in square formation, the medical aid, of course, was all inside the square, and was practically all dressing stations; and in bush fighting, as in the west coast of Africa, there can be no stringing out of the medical aid, all must follow on close behind the troops.

In South Africa when the Army got into the open plains of the Free State and of the Transvaal, the fact that the ambulance wagons could go practically everywhere and were not dependent upon roads caused the system I have described to be considerably modified, and what very frequently happened was that the collecting and dressing stations were not formed at all, but the wounded were dressed as well as possible where they lay and the wagons came right up to them and took them directly to the field hospital. To do this our own heavy and cumbersome wagons had to wait for a diminution or cessation of the firing, but the light and springy ambulances of the N.S.W. Medical Corps used to take down their Red

Cross flag and gallop about under fire picking up the wounded in ones and twos and taking their chance of a casualty. This method has its advantages, especially in a country where darkness soon comes on, but our wagons cannot possibly do it as they are at present constructed.

I shall not enter into details about the field hospitals, although they should be pitched as close as possible to the fighting and may be sheltered from fire only by the configuration of the ground and are, therefore, practically in the battle. Each is equipped for 100 patients, but as these hospitals are supposed to be mobile and to pass their sick and wounded as soon as possible to the hospitals on the lines of communications, they are not supplied with beds, or hospital rations. Each has tents and some surgical and medical equipment, but they have none of the comforts, or even the necessities, of a standing hospital. Like the bearer company, field hospitals are scratch formations thrown together on the outbreak of war and never seen in peace. In South Africa they were frequently called upon to perform work far in excess of that for which they were provided, with the inevitable result that perfection was not attained, and undeserved censure was passed upon their officers by persons ignorant or regardless of the real facts.

I trust, gentlemen, that I have not wearied you uuduly by this lecture, and that some of you who are not medical men have learned a little of Army medical matters. I have tried to show you that our organization is but a thing of yesterday, and that we are called upon to do in war what we are unable to practise in peace. A fierce light at present beats upon us, but though we are deficient in officers, in men, in material, and in opportunities for improvement, yet we believe we act up to our motto: "*In arduis fidelis,*" and we are proud of the way in which our representatives of all ranks in South Africa have shown that motto to be no empty phrase.

Reviews.

THE VENEREAL CLINIC: THE DIAGNOSIS, TREATMENT AND PREVENTION OF SYPHILIS AND GONORRHŒA. By several Writers. Edited by Ernest R. T. Clarkson, M.A.Cantab., M.R.C.S., L.R.C.P. With an introduction by Sir Squire Sprigge, M.D.Camb., F.R.C.S.Eng. Illustrated with 11 coloured and 9 other plates. Pp. xiii and 447. London: John Bale, Sons and Danielsson, Ltd. Price 25s.

This book, written for the student of venereal diseases, is divided into two parts—one deals with the clinical side of the disease, the second deals with administrative and social questions.

Syphilis and its treatment is condensed into seventy-two pages. The course of novarsenobenzol recommended is short, but the doses are too high to be compatible with safety, especially in an out-patient department. An initial dose of 0·6 gramme, followed by three weekly injections of 0·9 gramme, would render a patient who is intolerant to arsenic liable to serious toxic effects.

The anatomy of the genito-urinary tract is described clearly and in detail in thirty pages.

In the treatment of epididymitis the author omits the effective method of rapidly cutting short the acute stage by injections of sulfarsenol in small doses. It is highly appreciated by the patient who loses all pain within a few hours of the first injection.

An excellent chapter is devoted to the urethroscope and the normal and diseased appearance of the urethral mucous membrane, illustrated by coloured plates. The writer (Mr. Wyndham Powell) has adopted for his externally lighted urethroscope a new principle in operative work by fixing his instruments in a small tube inside the urethral tube and is thus able to use the body of the urethroscope as the *handle* of the cautery, curette or knife which is used.

A section is devoted to gonorrhœa of women and children, and two opinions are given as to the frequency or otherwise of urethral gonorrhœal infection in the female, in two different sections.

Part II of the book gives an up-to-date résumé of the sociological position of venereal diseases and its prevalence in the community. Chapter II discusses all the known methods that may be used in the control of venereal diseases and would be of interest to medical officers who wish to know the various problems of this controversial subject as it affects the civil community, which necessarily reacts on the Army.

The latter part of the book and the Appendices show how a civil venereal clinic should be designed, equipped and administered. A. V. F.

ATLAS OF SYPHILIS. By Professor Leo v. Zumbusch, Munich. 31 plates, 63 illustrations taken direct from nature by colour photography and one single colour illustration. London: John Bale, Sons and Danielsson, Ltd., 1922. Price 30s.

The direct colour photography used in the production of this atlas gives a realistic picture of the various common lesions occurring in syphilis. The selection of primary lesions is not good, as only two types are shown out of five recognized varieties.

The secondary and tertiary skin and mucous membrane syphilides are well reproduced and are characteristic.

Five plates are devoted to congenital syphilis of which two show active disease and three the results of bone infection.

The book would be of more use in civil clinics than in military venereal work where few of the later lesions depicted are met with at the present day.

A. V. F.

Correspondence.

THE FILTER-PASSER OF INFLUENZA.

(A RESPONSE TO COLONEL GORDON'S REPLY.)

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—Perhaps I may be allowed here to thank Colonel Gordon for his most courteous reply to my criticism of the filter-passing organism of influenza. It is greatly to my distaste that the work upon which I am now engaged involves so much criticism of the views of others, but I am unfortunately situated. To gain my objective, I have to carry strongly entrenched positions; whereas, if I had only commenced this study a few years earlier, I should have had a position of my own to consolidate against attack, and I do not think *nearly* as much would have been heard of all these various granular bodies as living organisms. I am relieved to find that Gordon does not think my criticism uninstructive; and,

indeed, I take courage from his able reply, because I think this shows a realization of the serious nature of my criticism. Honestly, my object is not merely to try and show that others are mistaken; far from it. It is because I envisage the entire subject—all the points with which I have dealt in various papers—as the manifestation of different phases in one great and extremely important mode of cell-behaviour, which has not been, I think, sufficiently grasped, that I have such doubts about these granules. It may turn out eventually, of course, that I am wrong; from the nature of the case, absolute proof, for either side, is extremely difficult to obtain. All that I can say is that, so far, I continue to obtain further evidence, from time to time, supporting, I consider, my view. But both sides have, equally, only the desire to ascertain the real truth; and, whatever the result, no discredit will attach, in such difficult cases, to the side which proves, ultimately, to be mistaken.

Colonel Gordon is a far more skilful fighter than I am, and fortunate in the possession of a more varied armoury of weapons; I am only extremely sorry that he is not on my side. But it so happens that my chief arm, and one to which I am well accustomed, is, when all is said and done, that upon which most reliance *has* to be placed in this particular kind of fight, so far as our methods for conducting such have yet progressed; though, as a good defender, Gordon does his best to minimize its value and uphold the strength of his other weapons.

Now the gravamen of my attack is *not* that the minute bodies in question stain red with Giemsa. It is rather that, *because they stain red with Giemsa, this is not to be taken as necessarily indicating a chromatinic nature.* No one, I think, holds that red coloration with Giemsa necessarily disqualifies such bodies from being those of an organism.[1] The important point, the point I meant to emphasize, is that neither does this necessarily disqualify such elements from being *other things than living organisms;* on the contrary, as I have been at some pains lately to show, various other protoplasmic structures, or products of cell-activity, also have a strong affinity for the "Roth aus Methylen-Blau." I think it will be agreed that protozoologists accustomed to the study of blood parasites have at least as many opportunities for realizing the characteristics of this method of staining as any other workers. And for many years past, the voice of the protozoologist has been as that of one crying in vain in the wilderness upon this matter—a matter far more important than many as yet recognize. The late Professor Minchin, in his great work on the cytology of *Trypanosoma lewisi,* was the first to point this out. And I take the following sentence from a paper by Wenyon (on the nature of certain granular bodies met with in kala-azar, *Journ. Trop. Med.*, 1914), which I happen to have lying by my hand in connexion with some work on "*Rickettsia*"-bodies which I am now writing up: "Another point which must not be forgotten is that many kinds of material other than chromatin are coloured red with the Romanowsky stain and its modifications, so that one must not be too hasty in jumping to the conclusion that every red-staining granule is chromatin." But this point *is* continually being forgotten or overlooked; and it is my firm belief that no real progress is possible in this granule-question, until it is driven home to all workers upon the subject. As it is, the field of protozoology is strewn

[1] I am afraid that a loose wording of my sentence may have misled Gordon. I ought to have written "not everything that stains red with Giemsa is chromatin"; though I had hoped that my meaning would have been understood, because I should have put "anything that stains red with Giemsa is not chromatin," had I intended as Gordon has evidently read it.

with the relics of encounters in which such granules have been upheld, unsuccessfully, as being chromatin pertaining to living organisms, by one side (e.g., Theiler's "*Anaplasma*"-bodies, Calkin's "*Cytorhyctes*," Seidelin's yellow-fever bodies, "*Paraplasma*," to name only one or two instances); and the field of these virus diseases alleged to be due to minute granular organisms will be, I fear, similarly littered.

A word or two only, here, about the "red" coloration, because I refer to this point in my forthcoming paper. By "red" is meant, broadly, containing the red element of the stain—the elusive "Roth aus Methylen-Blau"; there may be more or less of the blue element present in addition, when the result is anything from a delicate lilac up to a mauve or purple.[1] The exact tint depends on so many factors which influence the staining result by this method. As I have previously pointed out, Giemsa is a most paradoxical stain and therein "lies the secret both of the success and of the pitfalls attending its use." Nevertheless, there is a distinct difference in the staining of these granular elements and that of ordinary bacteria, as I showed, and, as I gather, Gordon agrees. Incidentally, I note that Rocha-Lima, in his account of "*Rickettsia prowazeki*" (1916) is considerably exercised by this very difference. As regards the point that the filter-passing granules do not stain with eosin, while the protein deposit does, this merely shows that different proteins behave differently in this respect. The inclusions in the Kurloff-bodies and Negri-bodies, which are certainly not organisms, but proteid substances derived from the hæmoglobin of ingested red corpuscles, also do not stain with eosin alone, though they appear a strong ruby-red after Giemsa.

As regards Colonel Gordon's figures to which I referred, I did not mention fig. 1, just because it did not illustrate my particular point. Moreover, the preparation from which it was taken was not quite comparable to that showing the secretion appearances, because it was from a greatly diluted culture. And I think it is quite possible that in such, granules of different sizes might tend to settle at, or pass to, different levels, according to their size. At any rate, in the corresponding fig. 1 which Gordon gives in his plate in the *British Medical Journal* of August 19, the granules are again fairly uniform in size, but here they seem to be distinctly larger than those of his fig. 1 in this Journal; and the magnification is given as the same in both cases. Further, Colonel Gordon will pardon me if I still think that, after leaving out the two individuals of *B. septus* in his fig. 2 referred to, which I compared with my fig. 8, there is, nevertheless, a considerable range of size in the case of the granules (unless the larger ones, especially in the lower half of the field, are also something different?) On the other hand, I refrained from alluding to his fig. 3, in which is shown a polymorph containing a few granules considered to be the phagocyted filter-passer. I confess I do think that Colonel Gordon is here trying even the faith of believers rather high. In any blood-smear polymorphs can be found showing a few more conspicuous red-staining granules. Personally I should be much more inclined to look upon the particular granules figured as the remains of digested bodies of phagocyted bacteria.

[1] I may again refer to the useful illustration of the nuclei of leucocytes. Some are a much deeper purple, others may be a brick-red; and this applies, it must be remembered to the whole nucleus, *including the large quantity of non-chromatinic, protein constituents.* Further, the same type of nucleus may vary appreciably in tint at different times, or in different cases.

I am afraid that I cannot, within the limits of this letter, consider all Colonel Gordon's points *seriatim*, or else it would be unpardonably long.

Really the matter amounts to this: regarding this granule-question as a whole, I fail to see where one is going to stop. Even *my* imagination rather staggers at the thought of what may yet be claimed as a filter-passing organism when occasion appears to require, and of the ubiquitous and teeming multitude of hitherto unsuspected living organisms now being revealed. To me this superabundance is rather a suspicious and disconcerting character than otherwise.

After all, the real crux of my argument was the fact that similar granular elements are produced as a result of the destruction, or lysis, of various kinds of cellular element; and just in such a case such abundance is far more comprehensible. Now this most important point is not discussed at all by Gordon. Does he agree that such granules are produced, on a large scale, in this manner? I am very glad to note that a reviewer has admitted that this question deserves, perhaps, more consideration than it has yet received. I myself think it is the key to the whole subject. All that Gordon mentions is the amorphous protein deposit. Whereas these granular end-products of cell-destruction (karyolysis), blood digestion and so on have a most definite *μορφή*, simulating to a remarkable extent different forms of bacteria—cocci, diplococci, bacilli, or even curved rods, as the case may be. Colonel Gordon does not answer my question as to how the filter-passing organism is to be distinguished from such discrete particles. I will only add that, once the above admission is made, as I think it will have to be, I entirely fail to see how the alleged cultivation of these granules as living organisms can have, *per se*, any evidential weight, since the cells of the portions of tissue necessary for the culture-medium will themselves be giving rise to similar granules in the course of their own death and disintegration.

Colonel Gordon dismisses my enzyme-hypothesis as ingenious but, in his opinion, highly improbable. I would very much like to ask him whether he regards the bacteriolytic principle of Twort-d'Herelle as a bacteriophage or as a bacteriolysin? Because I consider that this case is particularly cognate to the subject under discussion. Here, too, the condition can be experimentally produced at will, but in spite of this fact very few, I believe, admit that it proves that the active principle or "virus" is a living organism, as d'Herelle contends. Therefore I do not think that the fact that influenza can be experimentally produced by means of filtered secretions or cultures does necessarily imply the existence of a distinct filter-passing organism.

Colonel Gordon says he is inclined to feel somewhat contemptuous of enzymes. But I could not help noting that, nevertheless, he is careful to suggest that the filter-passers themselves manufacture enzymes. It rather looks as though Gordon felt none too confident in the ability of his filter-passer to make itself inconvenient, notwithstanding the vast numbers in which it is present, and thought that, at any rate, there would be no harm in endowing it, for luck, with any pathogenic assistance that the despised enzymes might be able to give. I shall not be surprised if the day comes when Colonel Gordon has a far greater respect for enzymes. Meanwhile I hope he will forgive me for this little "riposte" in return for his gentle thrust at the morphologists.

I am, etc.,

December 11.

H. M. Woodcock.

THE IDENTITY OF ALASTRIM.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—Some interesting clinical observations on alastrim by Major W. F. M. Loughnan appeared in the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS of November, 1922. In this article the author refers to the necessity for making a differential diagnosis between alastrim and various other conditions associated with cutaneous eruptions, but more especially between alastrim, smallpox and chickenpox, and quotes a table illustrating the differences observed in these three diseases.

I venture, in view of more recent work on alastrim which has come to my notice, to point out that experimental research in America has indicated that this disease is nothing more or less than modified smallpox. It is not surprising that Major Loughnan has overlooked these observations, as the difficulty of obtaining access to current literature when one is serving abroad and is divorced from a well-stocked library is inevitable.

The American work[1] to which I refer, was directed to obtaining experimental evidence of the identity of the virus of alastrim with that of smallpox. The following is a very brief résumé of its results:

(1) A vesico-papular eruption in monkeys was produced by inoculation both with crusts and with vesicle contents from cases of alastrim arising in Jamaica and Haiti. It was subsequently shown that these animals were protected against re-inoculation with alastrim and vaccine virus.

(2) Rabbits inoculated with alastrim showed no eruption, but were almost completely immune to vaccine virus.

(3) Rabbits previously inoculated with vaccine virus yielded positive intracutaneous reactions to subsequent inoculation with smallpox crusts, alastrim material, and vaccine virus, but gave negative reactions when material from chickenpox vesicles was employed.

These immunological reactions, together with the fact that previous inoculation with alastrim material produces a definite immunity to vaccinia, is very strong evidence of the essential identity of alastrim with smallpox.

Confirmation of this work has come from Canada[2] where a mild type of smallpox has been experienced for the last few years, and has been described by various names, such as alastrim and pseudo-smallpox. This research was undertaken with the view of obtaining some laboratory test which would help in the difficult question of differential diagnosis between modified smallpox and chickenpox.

Rabbits were scarified on the abdomen and inoculated with vaccine virus. Successful development of vaccinia led to sensitized condition of the animal, so that the subsequent intra-cuticular injection of either vaccine virus or of the contents of vesicles from smallpox patients was followed by the development within forty-eight hours of a red papule at the site of injection. The contents of chickenpox vesicles yielded consistently negative reactions on inoculation. It is

[1] Leake, Public Health Reports, United States Public Health Service, 1921, 36, 25, p. 1437.

[2] Defries and Hanna, "Studies from Research Division, Connaught Antitoxin Laboratories, University of Toronto," 1922, 1, p. 112.

concluded that this method affords a reliable laboratory test for the diagnosis of smallpox.

In conclusion, I might add that in view of the recent mild outbreak of smallpox in this country, rabbits have been sensitized in the above manner at the Royal Army Medical College, and the experimental work that has so far been undertaken is in substantial agreement with that quoted above.

Royal Army Medical College,
London, S.W. 1,
November 22, 1922.

I am, etc.,
H. Marrian Perry,
Lieutenant-Colonel, R.A.M.C.

Notices.

EDITORIAL NOTICES.

The Editor will be glad to receive original communications upon professional subjects, travel, and personal experiences, etc. He will also be glad to receive items of news and information regarding matters of interest to the Corps from the various garrisons, districts, and commands at home and abroad.

All such Communications or Articles accepted and published in the "Journal of the Royal Army Medical Corps" will (unless the Author notified at the time of submission that he reserves the copyright of the Article to himself) become the property of the Library and Journal Committee, who will exercise full copyright powers concerning such Articles.

A free issue of twenty-five reprints will be made to contributors of Original Communications and of twenty-five excerpts of Lectures, Travels and Proceedings of the United Services Medical Society.

Any demand for *reprints, additional to the above,* or for excerpts must be forwarded at the time of submission of the article for publication.

Matter intended for the Corps News should reach the Editor not later than the 15th of each month for the following month's issue. Notices of Births, Marriages, and Deaths are inserted free of charge to subscribers. All these communications should be written upon one side of the paper only; they should by preference be type-written; but, if not, all proper names should be written in capital letters (or printed) to avoid mistakes, and be addressed: The Editor, "Journal of the Royal Army Medical Corps," War Office, Whitehall, S.W.1.

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Journal

of the

Royal Army Medical Corps.

Original Communications.

"RICKETTSIA"-BODIES AS A RESULT OF CELL-DIGESTION OR LYSIS.

BY H. M. WOODCOCK, D.SC.LOND.
Fellow of University College.

I HAD not intended to consider the question of the "Rickettsias" in blood-sucking invertebrates, but I have lately observed such illuminating instances of their production, as I regard the matter, that I feel constrained to refer to the subject. It is with extreme reluctance that I find myself holding a divergent view from that of many authorities on the "Rickettsias"; but I am convinced that, in the wholesale discovery of new "species" to which we are now being treated (the most recent being still another by Sikora [15] of a form from "bird-lice") not nearly sufficient attention has been given to the question of the granular end-products of the digestion or lysis of cellular elements. I think, therefore, it is most necessary that one or two illustrative cases of this mode of behaviour should be also considered in this connexion.

I.—ON THE PRODUCTION OF "RICKETTSIA"-BODIES *EN MASSE* BY THE DISINTEGRATION OF *TRYPANOSOMA MELOPHAGIUM* IN THE SHEEP-KED.

I received recently a copy of a most interesting paper by Hoare [3], in which this author shows conclusively that "*Crithidia*" *melophagia* is only the crithidial phase of the life-history in the sheep-ked (*Melophagus ovinus*) of the sheep trypanosome, *T. melophagium.* As I was myself greatly interested in this point several years ago, in consequence of my discovery of the sheep-trypanosome, I took an opportunity, one leisure hour, of looking again over some of my old preparations from the alimentary tract

of sheep-keds, to see if I could find the important trypaniform phase—in all probability, the transmissive phase—which everyone had missed until it has now been recognized and described by Hoare. I had not been looking long, however, before my attention was completely diverted by the observation of the occurrence which I propose to describe. Little description, however, is necessary, because, as I think will be agreed, the excellent photomicrographs "speak" for themselves. I desire to express my grateful thanks to Dr. D. J. Reid and to Mr. A. Dennis for kindly taking the same for me.

The preparations from which the figures are taken are all smears stained by Giemsa; and although they are eleven or twelve years old, it may be mentioned that they are still as good as ever. The figures throughout are at a magnification of 1,000.

By way of preface, it is advisable, perhaps, to draw attention to two well-known features which are often presented by hæmoflagellates. One of these is the phenomenon of agglomeration, in which the parasites become massed together in rosettes or clusters; this has been often described, of course, in the case of trypanosomes. The exact significance of this tendency to clump together is not known; but it certainly represents a particular form of response to the onset of unfavourable conditions in the environmental medium in which the parasites happen to be, which affect the latter in a detrimental manner. Left to themselves such agglomerations invariably die, the more central individuals being the first to do so. Now, in the mid-gut of a sheep-ked, for instance, which has a heavy infection with "Crithidiæ," there is frequently a great tendency for this process to occur. If the gut-contents of such a ked are examined in a fresh condition, these clusters may present a striking picture, the larger ones being visible to the naked eye.

The second point is the occurrence of characteristic small granules in the cytoplasm of the parasites; these granules have been called "chromatoid," or "metachromatic," chiefly because they stain red with Giemsa. These granules, too, have been often described in trypanosomes, etc., especially in the case of involution forms, i.e., altered and manifestly unhealthy individuals. I shall refer more particularly to a few examples later on. Such granules are to be regarded as products of some abnormal form of metabolism, resulting most probably from an unfavourable condition of the environment. As Minchin has pointed out, they are most probably distinct from "volutin"-grains, which represent, on the other hand, a true store of reserve food-material, for nuclear use during the course of the normal life-history. The granules with which we have here to deal may, or may not be, in this early stage, actually derived from the nuclear material;[1] but at any rate they indicate already some derangement of the

[1] I am more inclined to consider that they represent unsuccessfully metabolized and non-assimilated nutriment (blood) absorbed.

normal metabolic activity of the nucleus. It need hardly be added, I think, that to-day no one holds the view that they are chromatin and represent living elements—"buds"—which can be extruded and develop anew into fresh trypanosome individuals.

In smears from the mid-gut of keds heavily infected with *T. melophagium*, both the processes to which I have referred may be in operation to an enormous extent; and the end-result is that the crithidial forms of the parasite *break down completely, giving rise to clumps, or dense masses of minute almost uniform, red-staining granules*, representing a few, or large clusters, of the parasites, respectively.

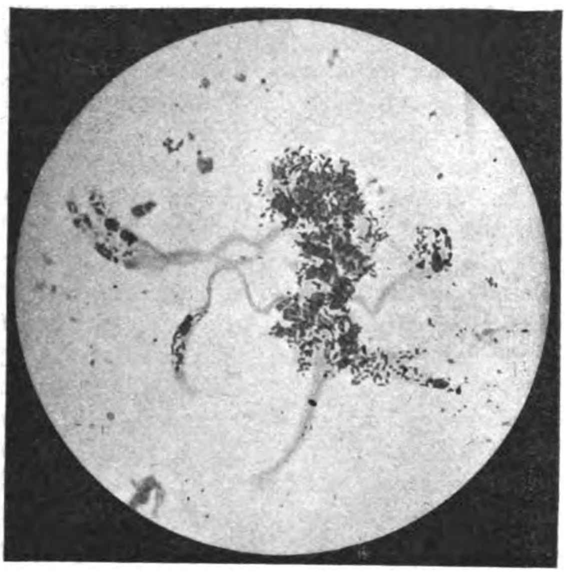

Fig. 1.

Figs. 1 to 3 are taken from such a smear. Throughout the smear, a vast number of crithidial forms are present, occurring either free or in clumps; the free individuals often tend to lie in twos or threes, parallel one to another. The characteristic binucleate condition can be readily made out. The kinetonucleus is the intensely stained body, usually oval, or even dumb-bell shaped (appearing black in the photos), situated in front of, i.e., on the flagellar side of, the larger, oval trophonucleus. Even as regards the individuals still possessing the normal shape and appearance, very few are entirely free from granules; the great majority contain either a few or else many.

Although in this smear there are still numbers of individuals to all appearance at the moment active and healthy, nevertheless cell-death and destruction are everywhere occurring on a large scale; not only in the case

of the agglomerated masses, but also in the case of solitary individuals and groups of a few. It would be interesting to know if the whole process is due to a more active display of resistance on the part of the host, e.g., that the alimentary tract is secreting some ferment which is affecting the parasites; or whether it is due to a form of autolysis on the part of the parasites themselves, owing it may be to their vast numbers, the share taken by the host being a passive one. Unfortunately, however, I have no means of determining the point; although I am inclined to think the digestive juices of the host are, at least, partly responsible. But there the interesting fact remains, that this enormous concourse of crithidial parasites in the mid-gut is for some reason becoming rapidly decimated.

The course of the breakdown, or lysis, of the parasites follows one of two rather different lines. Taking first that which appears to be much the more general in this smear, this is characterized particularly by disintegration and disappearance of the cytoplasm, and in consequence the body loses its definite form and appearance. Concurrently, the number of the granules becomes greatly increased. The granules represent, therefore, in the first place, the visible remains of the lysis of the cytoplasm of the parasites. Because for some while the nuclear elements persist, particularly the kineto-nucleus, which is the last portion of the parasite to remain definitely recognizable. Frequently, in a cluster of granules resulting from the dissolution of a group of parasites, a few odd, persistent kinetonuclear elements can still be recognized amongst them, after the trophonuclei have also completely broken down. As already indicated, this process of resolution into granules is taking place equally in the case of isolated individuals, or groups of two or three (fig. 1, right-hand side), and in the case of agglomerations, small and large. In the case of large clusters (fig. 8), the central part is a dense mass of granules, while all round the periphery are individuals which have only recently become attached, and still retain their recognizable form.

One particularly interesting point to be noted is that, where several parasites lying side by side, parallel to one another, have become completely broken down into granules, the granules have a very definite row-like arrangement (cf. right-hand side of fig. 8).

In the other variety of the process, an appearance which is, at first sight, very curious is to be observed. This condition does not happen to be nearly so manifest in this particular smear, and I shall deal with it more fully in the second part of this paper. Here and there occur irregular masses of blue-staining cytoplasm, to which are often closely attached, or rather united, "Crithidiæ," in greater or less number (fig. 9). Some of the parasites appear normal, but others have lost their typical morphological characters and are breaking down. The important point to notice is that, in this cytoplasmic matrix can be seen, not only persistent nuclear remains, but also the characteristic granules, either in clumps or scattered about. The chief difference between the two cases is that, in the true agglomeration clusters, the central part is a mass of nothing

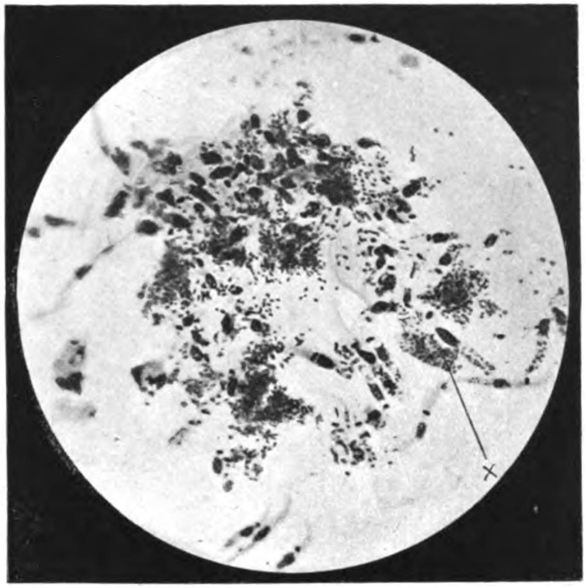

FIG. 2

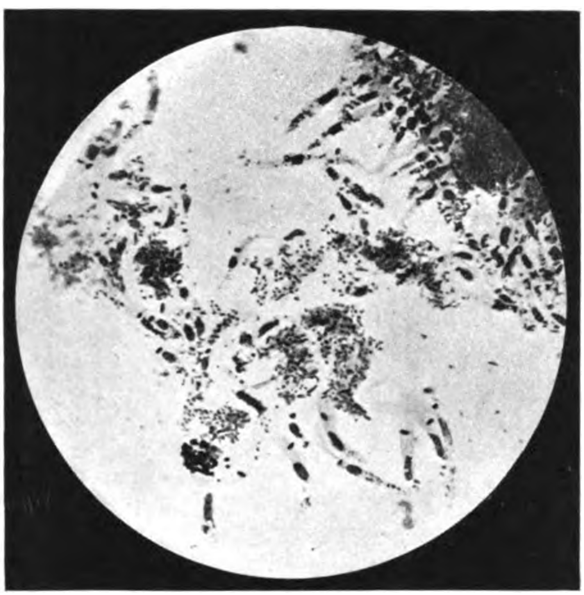

FIG. 3.

To illustrate "'Rickettsia' Bodies as a Result of Cell-Digestion or Lysis," by H. M. WOODCOCK, D.Sc.Lond., Fellow of University College.

FIG. 4.

FIG. 5.

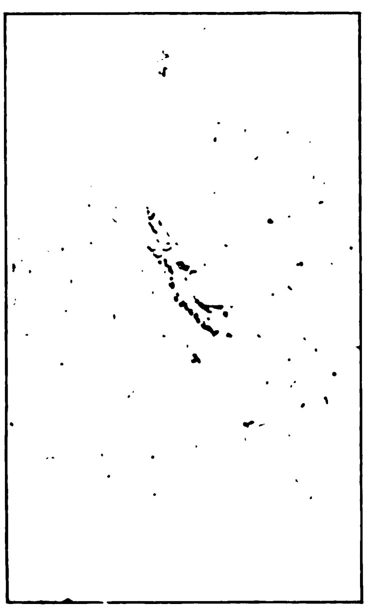

FIG. 6.

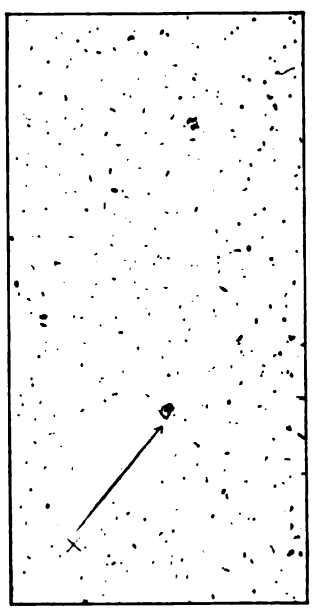

FIG. 7.

FIG. A.

To illustrate " Rickettsia' Bodies as a Result of Cell-Digestion or Lysis," by H. M. WOODCOCK, D.Sc Lond., Fellow of University College.

but granules, while here the granules occur definitely in cytoplasmic masses of varying size. I will only add now that these portions of cytoplasm are fragments of the epithelial cells of the digestive tract.

In a smear of the mid-gut of another ked I can find no parasites at all, that is to say recognizable as "Crithidiæ" by the usual characters. But throughout the smear are clumps and masses, of varying size, of granules, perfectly similar to those I have described; and, of course, isolated granules are scattered about everywhere. And if I had not the key to their origin, they might perhaps be taken, at first sight, for organisms. But looking carefully at what is present, it becomes clear that there *has* been a heavy infection with crithidial parasites, which, so far as can be seen, have all been destroyed. In the first place, dispersed everywhere (cf. figs. 10 and 11) are little but definite masses, staining a paler red, which represent undoubtedly the remains of the nuclei of the parasites, not yet completely broken down. Here and there (cf. *x*, fig. 11), a smaller, rather more deeply staining body is seen in close juxtaposition to a larger one, indicating, respectively, the kinetonucleus and the trophonucleus of a destroyed parasite. Further, there are numerous areas, smaller or larger, of the same blue-staining cytoplasmic matrix, to which I have above referred. These, too, often contain clumps of the granules, and now and again still recognizable nuclear elements (cf. *x*, fig. 10, where, again, a trophonucleus and a kinetonucleus are seen close together).

So much, then, for the mode of origin of an innumerable quantity of characteristic granules which may be found in the gut of a sheep-ked. As regards the granules themselves, these are, in my preparations, remarkably uniform. They appear as minute "cocci," or else rather dumbbell-shaped, almost like "diplococci." In the former case the diameter is from 0·3 micron to 0·4 micron; in the latter case the granules are about 0·5 micron to 0·6 micron by 0·3 micron. Occasionally a few slightly elongated forms are seen.

Next as regards their staining appearance. It is most necessary to bear in mind here the known variability of the Giemsa stain (or other Romanowsky stains equally) in regard to the exact tint in which the same thing may appear stained in different cases. I have already referred to this important point in my "Introduction to the Study of Hæmatophagy" [19]. Too much stress must not be laid on the question of the exact staining tint shown; and in any particular case, in judging the colour value, the appearance of the smear as a whole must be weighed, as affording an indication of the effects of the various extraneous factors (which need not be here instanced), well known to influence the general colour-picture. All these things have to be taken into account, if our interpretation of the results obtained by such a capricious and paradoxical but precious stain is to be of value.

The smear from which the figures showing both parasites and granules were taken well illustrates this point; the tint or tints of the same thing vary considerably in different parts of the smear. I have chosen most of

the figures from places where, as I have been accustomed to think, the parasites are most "nicely" stained; but here again the question of personal predilection also comes in. Where, for instance, the cytoplasm of the "Crithidiæ" has a very faint tinge of mauve or lilac, the trophonucleus being reddish-purple and the kinetonucleus very dark, almost a blue-purple, the granules are a bright red, with scarcely any lilac tint about them. In other parts, however (which might equally be regarded as "nice"), the cytoplasm is a purer blue, the trophonucleus darker, a deeper purple, and the granules are now distinctly lilac in tint (fig. 9 is from such a position). Again, in thick, denser parts of the smear, everything is much too blue, and, of course, as regards the granules, this is especially the case where they are aggregated into masses in the middle of a huge rosette, when they may appear frankly blue. Now for some reason in the smear from the other ked to which I have referred, where there are no "Crithidiæ" manifest as such, the granules in the smaller clumps are distinctly more lilac in tint, and the cytoplasmic masses are blue. I repeat, therefore, that in the circumstances great stress cannot be laid on the exact tint assumed by these granules; and there would be, of course, no justification for saying that because they appear red at one time or place, lilac at another, and even blue at yet a third, we have to deal in these different cases with different things. Those who have studied blood-smears will know that perfectly corresponding differences in tint are presented in different smears by the platelet-granules! To summarize the matter: where the general colouring is that which I myself am accustomed to associate with a satisfactory Giemsa-picture, these granules in this particular case are red to lilac in tint.

THE QUESTION OF "RICKETTSIA MELOPHAGI."

In 1917, Nöller [8] described bodies from the alimentary canal of *Melophagus ovinus*, which he called *Rickettsia melophagi*; and the same bodies have been subsequently described by Sikora [16] and also by others. The accounts by the two authors specified are very brief, and both are quite unillustrated; but the following important points appear to be generally recognized.

(1) The "organism" is a typical "Rickettsia"; thus, Nöller says, "Grösse und Morphologie etwa wie bei *Rickettsia prowazeki*" [i.e., the typhus-form]; and again, Jungmann [4] says, "In allen Fällen [i.e., in the keds examined] fanden sich sehr zahlreich morphologisch und färberisch in keiner Weise von der *Rickettsia prowazeki* und Wolhynica unterschiedbare Organismen." The "organism" is described as being, typically, like a coccobacillus or a diplococcus; and Nöller gives as its size in "cultures," 0·4 micron to 0·6 micron.

[1] The ked-granules are, if anything, rather larger than the louse-granules in typhus. This is apparent both from a comparison of Rocha-Lima's fig. 2 [11] of the latter, with my own photos of the former, at the same magnification; and also from the comparative representations of both given by Rocha-Lima [12].

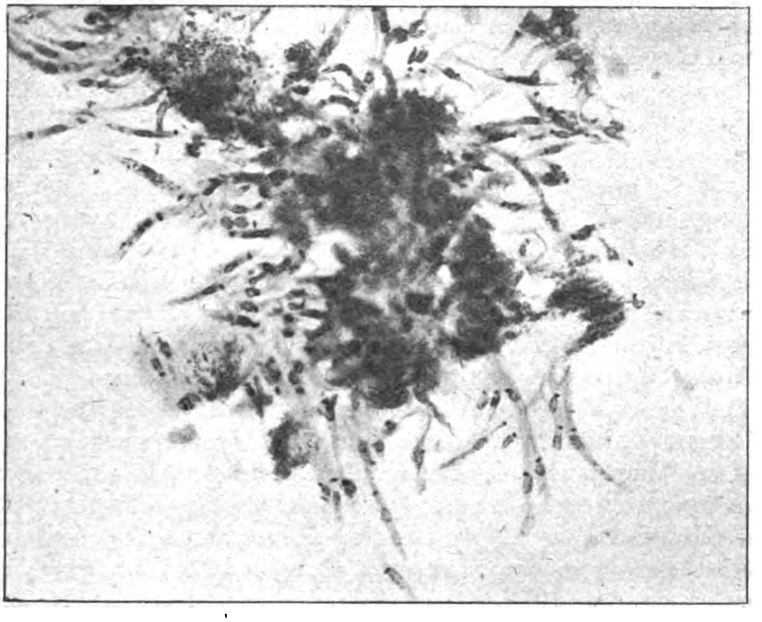

Fig. 8.

Fig 9.

(2) It is generally present, in smaller or larger numbers, in the mid-gut of the ked; and *its occurrence in close association with the crithidial parasites* is apparent from the various accounts. Thus Sikora says: "Während alte Melophagen sehr stark mit Rickettsien infiziert sind und überdies ungeheure Mengen von Crithidien beherbergen, enthält der Magen junger Melophagen . . . . sehr wenige Rickettsien." Again, Jungmann says: "Im Mittel und Endtheile des Darmes zeigen sie [the "Rickettsias"] eine characteristische Anordnung, sie treten in enger Verbindung mit den wie Pfeile in einem Köcher der Darmwand aufsitzenden Crithidien auf."

(3) A point of very great importance, commented on both by Sikora and Jungmann, is the very characteristic tendency of "*R. melophagi*" to be arranged in rows, like a palisade, vertical to the cells of the epithelium. Rocha-Lima [12] gives a diagrammatic sketch of this condition.

(4) The results of the "cultivation," as described by Nöller, are in my opinion most instructive. Both the crithidial parasites and the "Rickettsias" appear in the same culture-tube, and indeed the "mixed infections" appear to be separable only with great difficulty. Further, the same conditions favour, or retard respectively, an active increase in the two cases. At a lower temperature (22° C. to 25° C.) both things increase very slowly, though the Flagellates gradually gain upon the "Rickettsias," so that by repeated passages sub-cultures were obtained which were much richer in Flagellates and poorer in "Rickettsias." Nevertheless, by the particular method he was using in this case Nöller did not succeed in completely separating the two. On the other hand, at 28° C. or 29° C., both Flagellates and "Rickettsias" increased more rapidly. However, in one instance—a most interesting physiological case, as I regard it, quite comparable with the appearance of certain "mutations" or strains amongst bacteria—Nöller succeeded in obtaining a pure "race" of "Crithidiæ," with the sub-cultures of which, up to the time of his writing, no "Rickettsias" were associated; though, as he is wise to point out, the possibility of a surprise appearance of "Rickettsias" after months is not to be excluded. With this I quite agree, although for a different reason from that which Nöller has in mind.

(5) Jungmann states that in a certain percentage of cases he found the "Rickettsias" in an intracellular situation, i.e., inside the epithelial cells of the gut.

(6) Hereditary transmission of the "organisms" is claimed to occur, because both Jungmann and Sikora found them in young keds just emerged from the egg, or pupæ which had not yet sucked blood. In such cases, no "Crithidiæ" are present, because it is now known that certain early workers were mistaken in thinking that these parasites are transmitted hereditarily.

Now, from my own observations showing that masses of granules agreeing in all respects with, and indistinguishable from "Rickettsias"

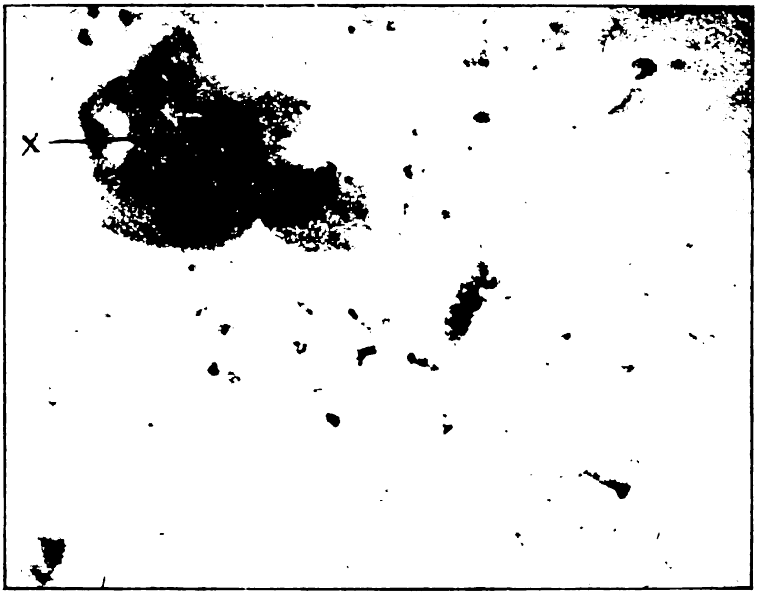

FIG. 10.

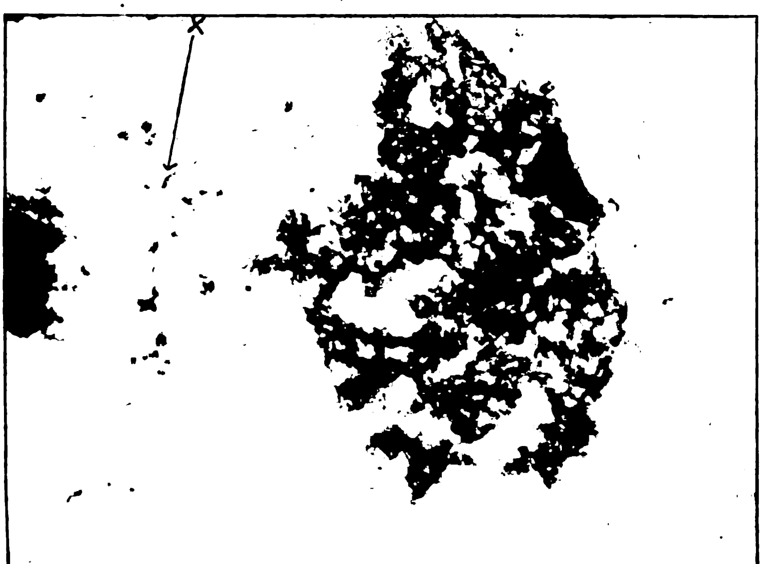

FIG. 11.

are produced by the disintegration of the crithidial forms of *T. melophagium* in the sheep-ked, and from the observations (1) to (4) above detailed relating to "*R. melophagi*," I consider that there is no such living organism; and that, in the vast majority of cases, the "Rickettsia"-bodies result from the breakdown of the "Crithidiæ."

It is really unnecessary to recapitulate the salient points of agreement, which will be apparent to all my readers. In regard to just one or two of these points, a word or two may be said. The tendency of intestinal flagellates of insects to attach themselves to the wall of the gut is well known. Naturally, therefore, if some disturbance of the balance between host and parasite occurs—most probably if the infection becomes too heavy for tolerance on the part of the host—and the parasites begin to succumb to the digestive juices of the ked, the row of individuals attached to the epithelial cells will suffer first. And it is clear from my fig. 8 that where parasites which happen to be lying parallel to one another become resolved into granules, these granules tend to be arranged in parallel rows. I must say, I do not think a better demonstration could be desired!

Again, as regards the "cultivation," the "Rickettsia" of the sheep-ked is that form which—to put the matter as optimistically as possible—has been most successfully "cultivated"; by "most successfully" I mean, that it has been "grown" without the addition to the medium of tissue-cell elements, which addition really vitiates the whole object of the culture, in the case of these granular bodies. Because, as I am glad to see it is now becoming more generally recognized, such elements themselves give rise to definite granules in the course of their own death and disintegration. But it so happens that it is just in this particular case that flagellates are also being cultivated at the same time—in the same tubes! Indeed, there is no evidence whatever that the "Rickettsias" have been cultivated *without flagellates either being present, or else having been present and having mostly or entirely died off*. And the "cultures" show most "Rickettsias" after two to three weeks' time! Now, is not that point most significant?

On the other hand, as mentioned, Nöller succeeded in obtaining a "strain" of the "Crithidiæ" in cultures which were free from "Rickettsias"; but that is an entirely different matter. Generally speaking, the healthy parasites do not show any granules—of course, here and there one or two can be found in the cytoplasm, but what I mean is, that so long as the metabolism is normal, there is no great increase in their number, as in the case of unhealthy and degenerating forms. Even in the smear from which my figures of the ked-parasite were taken, individuals can still be found which contain no granules.[1] And I think the explanation

[1] Such individuals may have become resistant, and serve to maintain the infection, or may pass on to the hind-gut, and there develop into the trypaniform phase, or (as in the case of *T. lewisi*), their descendants may again pass forward and repopulate the mid-gut

of the above case of Nöller's is that a more resistant strain of the parasites was becoming successfully developed, one more completely adapted to live and thrive normally in the new and unaccustomed environment of the cultures.

ANOTHER "RICKETTSIA"—UNWITTINGLY "CULTIVATED"!

I find that I, too, have all unknowingly "cultivated" a "Rickettsia," so long ago as 1913, and apparently a "form" hitherto undescribed. Looking over my preparations for the purpose of this paper, I came across a smear made from a culture of the true *Leptomonas* (*L. pattoni*) of the rat-flea (*Ceratophyllus fasciatus*); the source of this parasite was from an infected batch of fleas which the late Professor Minchin and my friend, Dr. J. D. Thomson, specially obtained in order to make a comparison with certain developmental stages of *T. lewisi* in the flea in the course of their epoch-marking research on this subject. I remember on one occasion trying to cultivate this Leptomonas in the blood-agar medium (of Novy and McNeal), which I was using in connexion with my bird-trypanosome work. This preparation caught my eye because it is labelled "granular forms"; the cultures (apart from this one) did not appear to thrive, and I could not obtain sub-cultures. This was apparently the only case in which considerable increase in number of the Leptomonads took place. Further, I remember distinctly being puzzled by the numerous, free little elements described below. Even then I did not think they were bacteria; they were quite unlike the usual bacterial contaminations with which, unfortunately, I had become only too well acquainted. I did not at that time think of connecting them with the granular elements in the parasites, and I put them down as some peculiar precipitation of the medium, because of their remarkably uniform dispersal throughout the smear; but I did not pay much attention to the smear, as soon as I found that the Leptomonads (on account of which it was made) were unhealthy. The smear shows no bacteria, and I regard it as having been made from a pure culture of the Flagellates.

Figs. 4 to 7 are taken from this smear; and here, again, these largely explain themselves. The Leptomonads are producing, in their cytoplasm, a number of minute, granular bodies, which, as the individuals break down and disintegrate, are set free into the surrounding medium (figs. 6 and 7). As will be seen, the granules appear very similar, in form and size, to those from the crithidial forms of the trypanosome in the ked. There is, perhaps, a slightly greater range. They begin as minute dots, which appear to be hardly more than 0·2 micron in diameter; but the majority are round or slightly oval, about 0·3 micron to 0·4 micron. On the other

with crithidial forms, when the conditions have become more favourable. Merely because in exceptional cases no living individuals are seen in the smears made, it by no means follows that the infection has entirely died out.

hand, there appears to be a larger proportion of distinctly elongated dumb-bell or diplococcal forms here; in such cases the entire length may attain 0·8 micron to 0·9 micron. Now and again they appear as three small cocci in a row (cf. about the middle of the stouter parasite in fig. 6). I have noticed this appearance in free forms also.

As regards their staining, the granules show a similar variation to that found in the case of the ked. In the case of those contained in the parasites, where the latter are well spread out, if anything rather flattened out, and their cytoplasm is a very delicate mauve, the granules are mostly red or slightly lilac in colour—even in such cases, however, some of them, especially the largest ones, are darker in tint—a dark red-purple, now and again almost as dark as the kinetonucleus (cf. the figures). But in Flagellates not flattened out, narrow forms, where the cytoplasm is a fairly strong blue, the majority of the granules are of this dark tint. On the other hand, the great majority of the free elements are of the red to lilac tint, although all transitions can be met with up to dark ones. A similar variation, it is important to note, is referred to and distinctly shown in the figures by Rocha-Lima [11], in the case of intra- and extra-cellular forms of *R. prowazeki.* There is, that is to say, no ground for considering that the elements in the Leptomonads are of a different nature from the free bodies. In the smear I have observed only three or four typical agglomeration-clusters; and in these everything is very blue.

There is one rather interesting point of difference in the case of the two parasites, i.e., the crithidial forms of *T. melophagium* and the cultural forms of *L. pattoni,* which well illustrates the difficulties attendant on judging from colours after the Giemsa-stain. In the latter case the trophonucleus is a much lighter colour (almost a light red) than it is in the first case—though in both cases the kinetonucleus and the cytoplasm are quite similar. For this reason, indeed, the trophonucleus has not come out at all well in most of the Leptomonads figured—though it is clearly distinguishable in the specimens themselves. If one did not know that we were dealing with the same thing in both instances, one might be reasonably excused for thinking that two different things were concerned! Hence, nearly all the granules in the Leptomonads appear stronger and darker in colour than the trophonucleus, whereas the opposite is the case in the "Crithidiæ." On the other hand, now and again it is almost difficult to distinguish the kinetonucleus from the larger diplococcal forms in the Leptomonads; and this point is interesting in comparison with the *Leishmania*-granules referred to below. In many cases, indeed, among the free elements some of the very dark forms are simply the slightly dumb-bell-shaped, kinetonuclear elements still recognizable as such, these being the last things to lose their distinguishing appearance.

But to return to the trophonucleus: it is most instructive to observe how as this "breaks down" and also becomes resolved into granular

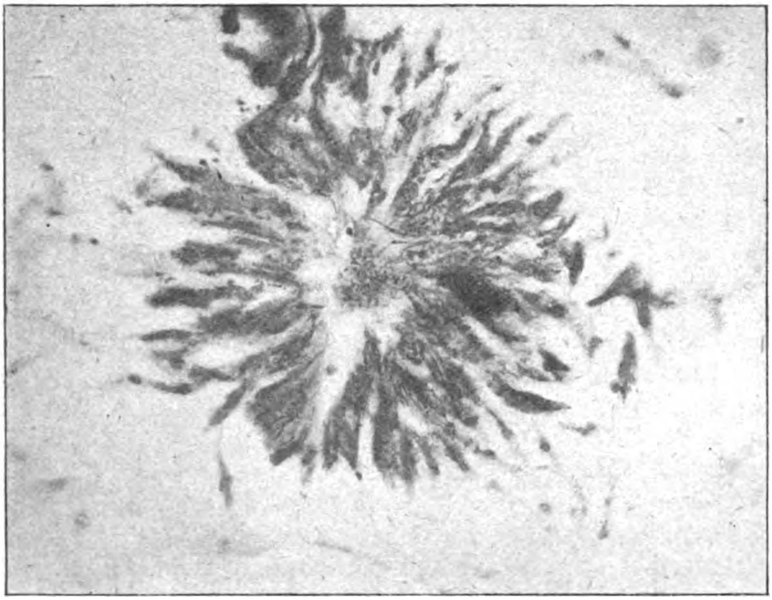

Fig. 12.

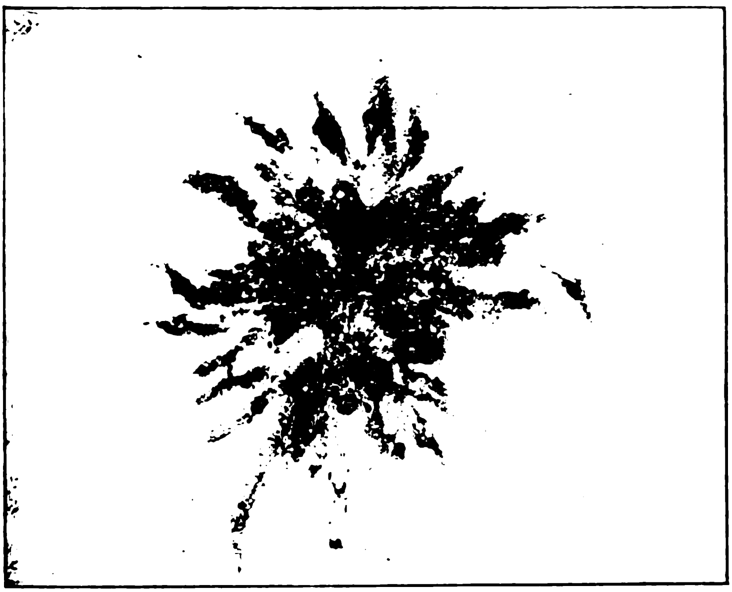

Fig. 13.

elements,[1] these take on both the same form and the same staining tint as the other granules all around. Whether it be the nuclei which have lost all their cytoplasm in the middle of the clumps of *T. melophagium*, or whether those of the little "boules" (i.e., the small rounded cytoplasmic fragment enclosing the two nuclei), representing the last distinguishable remains of the Leptomonads in this smear from my culture, there comes a point when it is also impossible to separate the trophonuclear granules from the surrounding free ones (cf. *x*, fig. 2, and again *x*, fig. 7).

Sikora mentions [16] that she obtained "Rickettsias" from a cat-flea, to which she gives the specific name of *R. ctenocephali;* she also saw "Rickettsia-like formations" in a mouse-flea, and refers the red-staining granules present in the Malpighian tubes to this "group." I do sincerely hope that no one will make another "species" of the granules which I have described in connexion with *L. pattoni*, from *Ceratophyllus:* and I should regard it as the extreme of irony if anyone called it "*R. woodcocki*"*!*

Now I think that what I have shown above is reasonably conclusive. There are, however, one or two possible objections which may be raised, though I myself cannot take them very seriously.

One thing that can be said is that the whole process I have described may be the result of an infection of the Flagellates with the particular "Rickettsias" of the two insects, the "Rickettsias" being parasitic in the "Crithidiæ" or the Leptomonads respectively, thriving and multiplying at their expense, and rapidly destroying them.

In reply to such an objection I point to the generally recognized fact, to which I alluded at the beginning of this paper, namely, that similar granules are of frequent and widespread occurrence in all these hæmoflagellates when they are degenerating in an unfavourable environment. I will give first a few references to which those readers who are desirous of so doing can turn. (Of course the hæmoflagellate literature teems with illustrations of these granules; I merely mention those which I have come across in looking over my memoirs at hand): Sleeping Sickness Reports of the Royal Society, No. 1 (Bruce and Nabarro), pls. 1 and 2; No. 4 (Bruce, Nabarro and Greig), pl. 1; No. 10 (Bruce, Hamerton,

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[1] It need hardly be added, I think, that these do not represent chromatin; the trophonucleus of the hæmoflagellates is of the typical "vesicular" character, with all the chromatin lodged in a central karyosome. The appearance of a mass of fine granules which this organella shows when stained by a Romanowsky method, is entirely due to the fact that minute protein particles in the nuclear sap have such a strong affinity for the "red" constituent of this stain that they become loaded up with it and usually hide the karyosome. As the nucleus degenerates and the chromatin becomes degraded, some chemical change in the general nuclear material takes place, which results in the formation of these discrete granular bodies, more solid than the karyolymph out of which they have been resolved (cf. my account of the disintegration of epithelial cells [20], and also the case considered in the second half of this paper).

Bateman, Mackie and Lady Bruce), pls. 4 and 5; No. 11 (ditto), pls. 4, 8 and 13; Blacklock (*Ann. Trop. Med. Parasitol.*, 6, pl. 5); and Behn (*Zs. Hyg.*, 70, pl. 7), (cultural forms of cattle trypanosomes). These figures all represent different trypanosomes, of man and animals (one is of a fowl trypanosome), either from the vertebrate host, or in cultures therefrom. The granules show in different cases just the same variations in form, size and staining appearance which I have indicated. Lastly, one of the earliest describers of "*C.*" *melophagia*, namely Pfeiffer [9] in 1905, figured the identical granules which I have above described, in the case of manifestly unhealthy and degenerating individuals (cf. his figs. 5, 6, 10 and 12).[1]

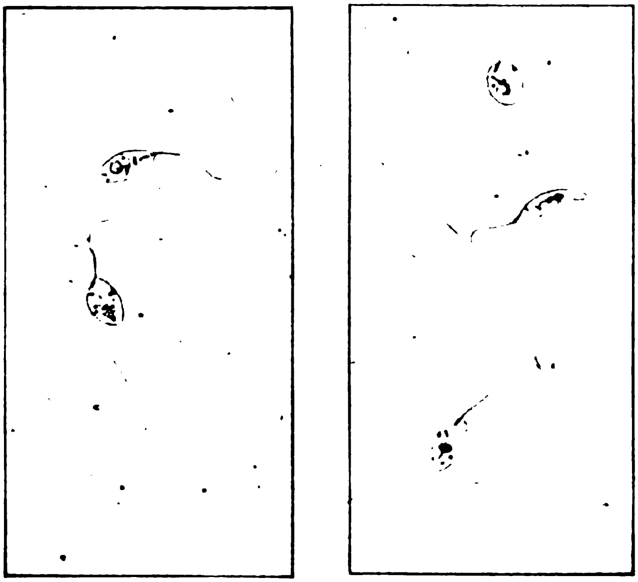

FIG. 14.

I had hoped to be able to show a similar breaking-down process in cultures of *T. melophagium* from the sheep direct, but unfortunately Dr. Hoare was unable to supply me with any at the moment. However, I looked up some preparations from a nineteen-day-old culture of a bird-trypanosome, in which I had noted the prevalence of granular and degenerated individuals. In one of these I found the two agglomeration-clusters of crithidial forms from which figs. 12 and 13 are taken. Although the preparation itself is, unfortunately, not good, yet the figures show

[1] Pfeiffer regarded these granules as an accumulation of reserve food-material, but this view is untenable where the parasites are breaking down. In such a case, they must rather represent incompletely metabolized blood, which the parasite is becoming increasingly unable to assimilate.

unmistakeably that the parasites in these rosettes can and do break down into a mass of granules beginning, here again, in the centre.[1] I do not say, of course, that these granules are necessarily of exactly the same size and chemical nature as the " Rickettsia "-bodies above described; for, in this case, we have to deal with a purely auto-cytolytic phenomenon. *But it is the same old type of granule!*

Lastly, Dr. Wenyon was kind enough to lend me a preparation of *Leishmania donovani* in culture, from which fig. 14 was taken. Here, again, are similar granules, markedly resembling the darker ones in the flea-leptomonad; it will be noted, especially, how often they have a pronounced dumb-bell shape, or diplococcal form; this is *no* exclusive criterion of a micro-organism; the kinetonucleus itself is frequently dumb-bell shaped, long before it is going to divide, and indeed may sometimes be double, or in three parts (e.g., in *Trypanoplasma*). In this case, also, some of the granules appear lighter in colour than the trophonucleus, while others, on the contrary, are darker; sometimes the latter approximate closely in appearance to the kinetonucleus.

No, it cannot be seriously maintained, I think, that these granules are parasitic " Rickettsias." If that were so, then ever so many vertebrates must be naturally infected with this remarkable organism; because, how, otherwise, could the trypanosomes, etc., become infected? Moreover, most of the authors above referred to do not consider, it may be pointed out, that a particular " Rickettsia " is a parasite of the vertebrate on which the particular insect feeds.

Again, it may be admitted that many of the granules, in the cases I have described, are associated with the lysis of the parasites, and yet it may be suggested that there are, *also*, real *Rickettsia*-organisms present. Well, of course, I at once agree that this would be very difficult to *disprove*. But this much can be said: *Having regard to the admission (ex hypothesi)*, such a suggestion would be a mere surmise, unsupported by any evidence whatever. And in reply I would simply ask if one of the essential postulates in connexion with the *vera causa* of the old logicians is satisfied, of the advisability of remembering which, in these difficult cases, we have been recently most pertinently reminded by my friend Dr. Ledingham. In view of the fact—for so, I think, it must be regarded—that masses of " Rickettsia "-like granules *are* being produced by the destruction of the Flagellates, *is it reasonable, is it a likely thing* to suggest that a micro-organism, *indistinguishable therefrom*, is also present? And that it is indistinguishable, not only to myself but to the previous workers, is manifest for the following reasons: (1) no one has separated the end-products of cellular disintegration from this organism; (2) all the authors, without exception, refer to the vast masses in which the organism is found at

[1] In this instance, the mass of entangled flagella and undulating membranes is apparently the first thing to break down.

times to occur. How and where, in fact, is a distinction to be made? In short, what definite indication, apart from its binomial appellation, have we of the existence, for instance, of *R. melophagi*?

But it will be contended that I have not taken into account points 5 and 6, enumerated above in connexion with the observations of other workers. Surely there is evidence here of the occurrence of an independent organism? I do not think so at all; but I have purposely left a consideration of these points until after I have considered other cases of the digestion of cellular elements.

EXPLANATION OF FIGURES 1 TO 14.

(All the figures are from Giemsa-stained preparations; and all are of the same magnification, 1,000 diameters. For description see text.)

FIGS. 1 to 3, 8 and 9.—*Trypanosoma melophagium* in the sheep-ked (*Melophagus ovinus*), showing different stages in the breaking down of the parasites, both singly and in agglomeration clusters, into "Rickettsia"-granules. Fig. 2, *x*.—A trophonucleus appearing now as a clump of uniform granules, indistinguishable from those in the cytoplasm of the same parasite. Fig. 3.—At the upper, right-hand corner is the edge of a large rosette. Fig. 8.—Note, at the right-hand side of the figure, that the granules are arranged in rows and represent the remains of a group of parasites lying parallel to one another. Fig. 9.—Parasites united to fragments of cytoplasm of varying size; in the latter occur groups or clumps of the granules.

FIGS. 4 to 7.—*Leptomonas pattoni* from the rat-flea (*Ceratophyllus fasciatus*), in culture, also breaking down into "Rickettsia"-granules. Figs. 4 and 5 show clumps of flattened-out parasites; fig. 6 a group of more normal shape. Note the gradations in staining intensity of the granules. Fig. 7 shows numerous free forms, regularly distributed. At *x* is a minute "boule," i.e., a portion of the cytoplasm of a parasite, rounded off and still containing the nuclear elements; the trophonucleus is resolved into granules similar to those around. Here and there are seen very dark elements representing free, still persistent kinetonuclei.

FIGS. 10 and 11.—To show cytoplasmic fragment containing aggregation of granules, also isolated, persistent nuclear elements, and masses of granules, resulting from the dissolution of crithidial forms of *T. melophagium*, in a smear in which no parasites as such are to be found. At *x*, in both figures, a trophonuclear and kinetonuclear element are seen in close contiguity.

FIGS. 12 and 13.—Agglomeration clusters of *T. fringillinarum* in a 19-days-old culture, showing the breaking down of the central parts into granules; the process has advanced to a slightly later stage in fig. 13 than in fig. 12. Ultimately, there would be nothing but a dense mass of granules to represent the cluster, as in fig. 8.

FIG. 14.—*Leishmania donovani* in pure culture (from a preparation kindly lent by Dr. Wenyon). To show granules in the cytoplasm of the parasites similar to those in the crithidial forms and leptomonads.

FIG. A (after fig. 7).—Blood-smear from a sheep (from a preparation kindly given me by Dr. Bedson). To show the platelet-granules, well marked and separate, for comparison with the "Rickettsia"-granules.

(*To be continued.*)

SEROLOGICAL EXAMINATION OF ONE HUNDRED STRAINS OF THE GONOCOCCUS ISOLATED FROM CASES OF ACUTE AND SUBACUTE URETHRITIS IN THE MALE.[1]

A REPORT TO THE MEDICAL RESEARCH COUNCIL.

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(*Concluded from p.* 24.)

VI.—REVIEW OF SEROLOGICAL INVESTIGATION OF THE GONOCOCCUS AND ACCOUNT OF METHODS EMPLOYED IN THE EXAMINATION OF THE SERIES UNDER DISCUSSION.

(*A*) *Short Review of Recent Literature.*

Review even of recent work dealing with the serological classification of gonococci indicates that considerable diversity of opinion exists concerning the relationship which various strains of the gonococcus bear to one another, and the relative frequency with which any given serological type of that organism is associated with gonorrhœa has not been so far determined in this country, although Hermanies has made an extensive examination of this kind in America.

Researches by Torrey (1907-1908) [18] and by Teague and Torrey (1907) [15] suggested that the gonococcus comprised a number of micro-organisms differing *inter se* in their antigenic properties in respect of agglutinating, complement fixing, and bactericidal antibodies.

In the series examined by Torrey such diversity was noted that it appeared improbable that sub-groups of the gonococcus could be clearly defined or the whole group identified by serological methods, but Torrey's series consisted only of a small number of strains and no attempt was made to determine how frequently any of the types was responsible for the causation of disease. Likewise Warren (1921) [19] came to the conclusion that the agglutination test does not serve to differentiate strains of gonococci into serological groups. On the other hand, Watabiki (1910) [19], working with only eight strains and employing the complement fixation method, divided these eight strains into sub-groups, but his results led him to regard the differences noted between these sub-groups as not distinctive but only comparative. More recently Jötten (1920) [11] investigated twenty-seven strains and by agglutination divided these into four sub-groups, of which five constituted this author's group A,

[1] With acknowledgments to the Editors of the *Journal of Pathology and Bacteriology*.

5 group B, 7 group C and 3 group D, while 7 strains could not be grouped. This investigator also traces a parallelism between agglutination, complement fixation and toxicity for mice, but unfortunately the description of technique in his paper is so meagre that repetition of his experiments presents some difficulty. Hermanies (1921) [10] gives an excellent account, with full description of technique, of the examination of eighty-five recently isolated strains by the absorption of agglutinins test and finds that seventy-six of these fall into one of two groups, forty-one being designated Type I and thirty-five Type II, by this author.

Gordon in a personal communication, January, 1921, informed me that on examination by the absorption of agglutinins test of thirty strains isolated in London, twenty-five of these owing to their close, though by no means clear-cut, relationship, as disclosed by this test, could be regarded as constituting a fairly well defined sub-group.

While Torrey and Jötten then lay stress on the multiplicity of types, Watakibi, Hermanies and Gordon call attention to the greater importance of one or two of these types. Hermanies and Gordon nevertheless admit that there are several types, many of which, however, appear to be of less significance owing to their being encountered much less frequently in cases of gonorrhœa.

(B) Preparation of Suspensions of Gonococcus for Agglutination Tests and for Immunization of Rabbits.

Cultures destined for agglutination tests were, after isolation, sub-cultured on the rabbit plasma agar already described, and after twenty-four hours at 37° C. the growth obtained was scraped off with an iron loop and transferred to a glass mortar with an accurately ground glass pestle. The growth was then triturated, suspended in saline and heated in a water bath to 60° C. for thirty minutes. Carbolic acid, five per cent solution, was then added in sufficient quantity to make the final concentration of that reagent 0·5 per cent, and the suspensions were finally standardized, by the opacity method, to contain 8,000 million cocci per cubic centimetre.

For agglutinations these standard preparations were used diluted with an equal bulk of 0·5 per cent carbolic saline, and for the absorption two cubic centimetres of the 8,000 million suspension was used for absorbing a definite number of "units" of the agglutinating serum.

It was found later that primary cultures could be used for agglutination and absorption of agglutinins tests, but on the whole these did not react to such high titre as did suspensions prepared from sub-cultures. Cultures destined for immunizing rabbits were prepared in the same way, but the strains used were sub-cultured for two or three generations in rabbit plasma agar before being cultured in bulk, on the same medium, for preparing the actual suspensions used for inoculation.

Medium enriched with rabbit plasma was chosen for this purpose

rather than unenriched medium, because of the greater luxuriance of the growths obtained and owing to the danger of loss of antigenic qualities, which many organisms are known to sustain if their culture in a simple medium entails a process akin to acclimatization.

In immunizing the rabbits a suspension of 16,000 million cocci per cubic centimetre is used and inoculation is made by the intravenous route in doses of 0·25 cubic centimetre (4,000 million), 0·5 cubic centimetre (8,000 million), 0·75 cubic centimetre (12,000 million) and 1 cubic centimetre (16,000 million), an interval of two days being allowed to elapse between each inoculation. The serum was sampled on the fifth day after the last injection and if its titre was 1/1,600 or more (i.e., a concentration of 1/1,600 or less sufficed to produce agglutination of the homologue in twenty-four hours at 55° C.) the animal was bled out under ether anæsthesia. The serum was separated and one-tenth of its bulk of freshly prepared five per cent carbolic saline was added as a preservative.

(C) *Action of Normal Rabbit Serum on Suspensions of the Gonococcus cultured and prepared as described.*

In view of the statements made by Hermanies and by Gordon that the gonococcus not infrequently agglutinates in presence of normal rabbit serum, six rabbits destined for the preparation of gonococcus agglutinating serum were each bled from the ear vein, and the serum so obtained was used for agglutinating sixteen different strains of gonococci in dilutions of 1/50, 1/100, and 1/200.

The technique employed in making these tests was the same as that described in the following subsection of this communication.

None of the sixteen strains tested showed agglutination in presence of any of the six sera in the dilutions employed. It must, therefore, be concluded that gonococci cultivated and prepared in the manner described do not agglutinate in presence of normal rabbit serum.

Notwithstanding this negative finding, a control tube containing a suspension of the strain under examination exposed to a 1/50 dilution of normal rabbit serum was always included in the routine tests. This only occasionally showed slight clumping which could, however, be easily distinguished from true agglutination with homologous immune serum of the same or much lower concentration. It was noted, too, that the more often a strain had been subcultured the greater tendency did it show to flocculate in presence of normal rabbit serum.

(D) *Technique of Agglutination and Absorption of Agglutinins.*

(i) *Agglutination.*—Only sera having a titre of 1/1,600 or more were used. The serum was distributed in a volume of 0·25 cubic centimetre into a series of four small tubes (three inches by three-eighths of an inch) and in such concentration that on adding an equal balk of suspension of

the organism to be agglutinated the strength of the serum was eight times, four times, thrice and twice the titre of that serum when tested against its homologue. A serum with a titre of 1/1,600 was therefore so distributed that, on adding the suspension of organisms, dilutions of 1/200, 1/400, 1/533, and 1/800 were obtained.

Incubation was at 55° C. for twenty-four hours, and the total volume of fluid in each tube was 0·5 c.c.

The suspension of organisms employed was of the strength of 4,000 millions per cubic centimetre, so that 0·25 cubic centimetre—the volume used for each tube in the test—contained 1,000 million.

(ii) *Technique of Absorption of Agglutinins.*—To two cubic centimetres of suspension of the organisms to be examined, the suspension being standardized to contain 8,000 million cocci per cubic centimetre, there was added sufficient serum to make the concentration equal to "16 T," i.e., if the serum used had a titre of 1/1,600 it was added in a volume of 0·02 cubic centimetre to the two cubic centimetres of suspension.

Sets of four strains and a control absorption with the homologue were tested at one time, so that adequate comparison of results was assured, and a tube containing three cubic centimetres of 0·5 per cent carbolic saline plus sufficient serum to make a concentration equal to 16 T was set up at the same time to serve as a control to determine the degree to which the strains tested were agglutinated in presence of the unabsorbed serum.

The mixture of serum and test organisms and the serum diluted in 0·5 per cent carbolic saline were incubated for twenty-four hours at 37° C., and the tubes were then centrifuged to deposit the organisms. The clear surpernatant fluid so obtained was then distributed for the second phase of the test. In carrying out the second phase of the test three sets of four tubes were used for the examination of each strain.

Into the first set of four tubes in each series was distributed the control (unabsorbed), serum in volumes of 0·25 cubic centimetre, and in such concentration that dilutions of 16 T, 8 T, 6 T, and 4 T were obtained. To each tube was added 0·25 cubic centimetre of a 4,000 million suspension of the organism under examination, giving dilutions of 8 T, 4 T, 3 T, and 2 T of the serum acting on 1,000 million cocci.

Into the second and third sets of four tubes in each series was distributed the absorbed serum in similar dilutions and volumes, and to each of the tubes of the second set was added 0·25 cubic centimetre of a 4,000 million suspension of the organism homologous with that used for preparing the serum. To the third set of four tubes, which also contain absorbed serum, is added a suspension of the strain under investigation. The tubes are then incubated for twenty-four hours at 55° C., each being plugged with cotton wool to prevent evaporation.

The first four tubes serve to show whether the strain under examination is agglutinated by the unabsorbed serum, the second four tubes show whether the coccus under examination removes all the specific aggutinins

from the serum, and the third set of four tubes shows whether the absorption was sufficient to remove all co-agglutinins, or, alternatively, whether the strain under examination tends to auto-agglutinate.

The method of absorption of agglutinins used in this investigation has been described in detail, as unless some standard method be adopted divergent results are liable to be obtained. The technique has proved extremely useful in the investigation of the meningococcus, the tetanus bacillus and members of the colon-typhoid group, and is a routine procedure in this laboratory.

(*E*) *Results obtained on Examination of Ten Strains, taken at random, by the Agglutination Method.*

Agglutinating sera were prepared by inoculating three strains taken at random, and when the titre of these sera had attained 1/1,600 the animals were bled out, and with the serum so obtained an agglutination test was carried out, the technique employed being that already described. The results obtained are shown in the following table :—

TABLE I.

Reference No. of case	Rabbit serum								
	V 1834			V 1885			V 1887		
	8 T	4 T	2 T	8 T	4 T	2 T	8 T	4 T	2 T
1834	++	++	+	++	++	+	++	++	+
1885	++	+	+	++	++	+	++	++	+
1887	++	++	+	++	++	+	++	++	++
1900	++	(+)	–	++	++	++	++	++	+
1901	++	(+)	–	++	++	+	++	++	+
1910	++	++	++	++	+	+	++	++	++
1913	++	++	++	++	+	–	++	++	+
1917	–	–	–	++	–	–	+	–	–
1927	+	+	–	++	+	+	++	++	+
1929	++	+	–	++	+	–	++	++	++

Note.—In this and in subsequent tables :—
++ = Complete flocculation.
+ = Flocculation visible to the unaided eye.
(+) = Flocculation demonstrable only by aid of a lens × 12 diameters.

The only comment which these results call for is that they are unexpectedly homogeneous, for only one strain, 1917, fails to agglutinate well in presence of any of the sera, while serum 1887 agglutinates all the strains tested excepting only 1917 when present in a concentration of 2 T.

(*F*) *Possible Fallacy in this Preliminary Test.*

The results of the preliminary test might be criticized on the ground that, as both the suspensions used for immunizing the animals and those

used in the tests were prepared from organisms which had not been subcultured more than twice on rabbit plasma after isolation in a medium containing human protein, the flocculation obtained was really a precipitin reaction. It is conceivable that a trace of human protein might be carried over from the primary culture, and acting as an antigen both in immunizing the animals and in the tests, might lead to false positive results being obtained.

Although this was but an improbable explanation of what took place it was decided to obviate this criticism by carrying out the following experiments:—

(i) To 0·2 cubic centimetre of the antigonococcus sera were added various dilutions of human serum—1/20 cubic centimetre, 1/200 cubic centimetre, and 1/400 cubic centimetre; these mixtures were then incubated for one hour at 37° C., but in no instance did even the faintest clouding occur, so that it was very improbable indeed that the routine findings were due to this mechanism.

(ii) The antigonococcus sera were each mixed with human sera and the mixture allowed to stand for twenty-four hours at room temperature, after which the tubes were centrifuged. No deposit was noted. The contents of these tubes were then used for carrying out agglutination tests, the homologous organism being exposed to each serum. The tests were done in duplicate, control (untreated), antigonococcus sera being compared with the same sera after exposure to human protein. The titre of the sera was in all cases unaltered by previous treatment with human protein.

The finding fully corroborated (i) and showed that the activity of the sera did not depend upon their containing anti-human precipitins.

(iii) In yet a third way could this criticism be dealt with: staphylococci and *B. coli* were grown on medium rich in human protein and suspensions were prepared from these in the same way as those used for the tests figured in Table I. Agglutination tests comparable in every respect to those under discussion were then set up, their suspensions being exposed to the antigonococcus sera.

No agglutination took place, thus confirming the results of experiments (i) and (ii) of this subsection of the report.

(G) *Specificity of Agglutination as a Means of Identifying and Classifying Gonococci.*

Owing to the homogeneity of the result given in Table I it appeared possible that agglutination alone might serve to identify the gonococcus. In order to investigate this the antigonococcus sera were mixed with suspensions of "type" meningococci, for if group agglutination of the meningococcus by antigonococcus serum was seen to occur it would be obvious that a further test—absorption of agglutinins—would be required to identify and therefore, *a fortiori*, to classify the gonococci.

The technique used was that already described, and the following result was obtained :—

TABLE II.

Meningococcus	Antigonococcal sera									Antimeningococcus sera			
	1834			1885			1887			Each against its			
	8 T	4 T	2 T	8 T	4 T	2 T	8 T	4 T	2 T	Homologue	8 T	4 T	2 T
Type I ..	+	+	–	++	+	+	++	++	+	Type I ..	++	++	++
Type II ..	+	–	–	+	(+)	–	+	(+)	–	Type II ..	++	++	++
Type III ..	+	–	–	–	–	–	–	–	–	Type III..	++	++	++
Type IV ..	+	–	–	(+)	–	–	+	–	–	Type IV..	++	++	+

There is therefore evidence of group agglutination when meningococci are exposed to the action of antigonococcus agglutinating sera, but it must be noted that in a series of crossed absorption tests in which the antigonococcus sera available were absorbed with type meningococci, and in a larger series of tests in which type antimeningococcus sera were absorbed with gonococci, in no instance was there observed any reduction in the titre of serum so tested in respect of its homologue.

It seems probable too that the group relationship which appears to exist between gonococci and meningococci depends upon peculiarities of the strains examined, and bears but little relationship to the specific serological type characters of the micro-organisms.

Thus Gordon in a personal communication informed me that in a series of similar tests conducted by him, Type IV meningococcus exhibited a particularly close relationship to the gonococcus, while Nicolle (1919), [14] obtained, after treatment of the suspensions by Porge's method, a similar marked group relation between the gonococcus and the parameningococcus B of Dopter which is said to differ serologically from all the type meningococci described by Gordon. In Table II the most marked agglutination is seen in the case of Type I meningococci exposed to antigonococcus serum.

As all these observers used different strains of the micro-organisms and different batches of sera in making their tests, the obvious explanation of the discrepancy in the results obtained is that group relationship between meningococci and gonococci is a factor peculiar to each strain and is not a factor of any serological " type " of either.

(*H*) *Results obtained on Applying the Absorption of Agglutinins Test to the First Series of Fifty Strains of Gonococci isolated.*

The results obtained in the previous subsection of this report clearly show that the agglutination test alone will not suffice to differentiate the various strains of gonococci and the absorption of agglutinins test was therefore adopted as a routine.

All strains isolated were in the first place used to absorb a serum prepared by inoculating culture No. 1887, as the preliminary test conducted with three antigonococcus sera (Table I, subsection E, *supra*) had shown that this was the least specific of the sera so far prepared and would therefore, presumably, be the most difficult to deprive of its antibodies.

The technique used was that described in Section VI—subsection D *supra*, and the results obtained with 1887 serum—hereafter referred to as Type I—may be thus summarized :—

"*Thirty-seven of these fifty strains very markedly reduced, or, within the limits of the experimental method employed, completely removed from this* ('1887') *serum the specific antibodies for its homologue.*"

The results of these absorptions may be tabulated as follows :—

TABLE III.—Absorption of "1887" Serum.

Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test cocci			
8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
Twelve strains reacted thus—											
++	++	++	++	–	–	–	–	–	–	–	–
Eight strains reacted thus—											
++	++	++	+	++	–	–	–	–	–	–	–
Five strains reacted thus—											
+±	++	+or++	–	–	–	–	–	–	–	–	–
Three strains reacted thus—											
++	++	+or++	–	+	–	–	–	–	–	–	–
Three strains reacted thus—											
++	++	+	–	++	–	–	–	–	–	–	–

These thirty-one strains appear, therefore, to be closely related in that they all very markedly reduce the titre of 1887 serum on applying the absorption test of Castellani (1902) [5] and using the technique herein described. There were five further strains in the series of fifty under consideration which considerably reduced the titre of this serum, but their agglutination reactions in presence of, and their absorptive capacities in respect of, this serum varied considerably; their reactions are therefore given in detail in the following table :—

TABLE IV.—Absorption of "1887" Serum.

Reference No. of strain	Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test coccus			
	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
1982 ..	++	+	–	–	+	–	–	–	–	–	–	–
2098 ..	++	+	+	–	++	(+)	–	–	–	–	–	–
2153 ..	++	+	–	–	+	–	–	–	–	–	–	–
2156 ..	++	++	–	–	++	–	–	–	–	–	–	–
2244 ..	++	+	(+)	–	+	–	–	–	–	–	–	–
Control 1887	++	++	++	+	–	–	–	–	–	–	–	–

These five strains, therefore, although they fail to agglutinate well in presence of "1887" serum, are seen to be endowed with very considerable capacity for absorbing its agglutinins and must therefore be considered as members of the same group.

One strain, No. 1917, was peculiar in that although it all but failed to agglutinate in presence of 1887 serum, it almost completely removed the agglutinins therefrom when tested by the absorption method. Its reaction is shown in Table V.

TABLE V.—ABSORPTION OF "1887" SERUM.

Reference No. of strain	Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test coccus			
	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
1917 ..	+	−	−	−	++	−	−	−	−	−	−	−
Control 1887	++	++	++	+	−	−	−	−	−	−	−	−

The remaining thirteen strains of this first series of fifty when exposed to, and when used for absorbing, "1887" serum gave the reactions shown in Table VI.

TABLE VI.—ABSORPTION OF "1887" SERUM.

Reference No. of strain	Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test coccus			
	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
1834* ..	++	++	++	+	++	++	++	++	−	−	−	−
1867* ..	++	++	++	+	++	++	++	++	−	−	−	−
1889* ..	++	++	++	+	++	++	++	++	−	−	−	−
1900* ..	++	++	++	−	++	++	++	+	−	−	−	−
2026 ..	++	+	−	−	++	++	++	++	−	−	−	−
2053 ..	++	−	−	−	++	++	++	++	−	−	−	−
2083 ..	++	+	−	−	++	++	++	+	−	−	−	−
2099* ..	++	++	++	+	++	++	++	++	−	−	−	−
2142 ..	−	−	−	−	++	++	++	++	−	−	−	−
2172 ..	++	+	−	−	++	++	++	++	−	−	−	−
2173 ..	++	+	−	−	++	++	+	−	−	−	−	−
2256 ..	++	++	++	−	++	++	++	++	−	−	−	−
2259 ..	++	+	(+)	−	++	++	+	−	−	−	−	−
Control 1887	++	++	++	++	−	−	−	−	−	−	−	−
Control 1885	++	++	++	+	+	−	−	−	−	−	−	−

Note.—Cultures marked (*) had been in stock culture for over a month before the tests were carried out, and it is not improbable that the frequent subculture which they had undergone had somewhat increased their agglutinability.

Only two of these, Nos. 2173 and 2259, show any absorptive capacity in respect of "1887" serum, and they may possibly bear some fairly close relation thereto, but the remaining eleven are sharply differentiated from

the thirty-seven strains whose reactions are dealt with in Tables III, IV, and V. These thirty-seven strains are, therefore, provisionally classed together as Type I.

A serum had also been prepared by inoculating a rabbit with strain 1834, and the above strains were tested to determine their absorptive capacities in respect thereof, the standard technique already described being used. The results obtained are shown in Table VII, and attention is called to the fact that strains 2153, 2156, and 2244, which failed to agglutinate well in presence of "1887" serum, were also included in the test, as also was "1887" itself to serve as a control.

TABLE VII.—Absorption of "1834" Serum.

Reference No. of strain	Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test coccus			
	8 T	4 T	8 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
Homologue 1834	++	++	++	+	−	−	−	−	−	−	−	−
1867	++	++	+	−	++	++	++	+	−	−	−	−
1889	++	++	+	+	++	++	++	+	−	−	−	−
1900	++	+	−	−	++	++	++	+	−	−	−	−
2026	+	(+)	−	−	++	++	++	+	−	−	−	−
2053	++	++	+	−	+	−	−	−	−	−	−	−
2083	++	++	+	−	++	(+)	−	−	−	−	−	−
2099	+	−	−	−	++	++	+	+	−	−	−	−
2142	++	+	−	−	++	++	++	+	−	−	−	−
2172	−	−	−	−	++	++	++	+	−	−	−	−
2173	+	−	−	−	++	++	++	+	−	−	−	−
2256	++	++	+	−	++	−	−	−	−	−	−	−
2259	++	++	+	−	−	−	−	−	−	−	−	−
Control 1887 ..	++	++	+	−	++	++	++	+	−	−	−	−
2153	++	++	++	+	++	++	++	+	−	−	−	−
2156	−	−	−	−	++	++	++	+	−	−	−	−
2244	+	−	−	−	++	++	++	+	−	−	−	−

Note.—Attention is called to the strain 2153 which agglutinates well in presence of "1834" serum, although it fails to absorb, while on the other hand, although it agglutinates but feebly in presence of "1887" serum, it does absorb the agglutinins therefrom. This strain had been in stock culture, however, for some time before the test under consideration could be carried out, and had in the interval been frequently subcultured.

It is seen from Table VII that five strains, 1834, 2053, 2083, 2256, and 2259, appear to be closely related, and are, therefore, provisionally regarded as constituting a second subgroup or "type."

These same strains were then used for absorbing a serum prepared by inoculating a rabbit with strain 1889, and the results shown in Table VIII were obtained.

Note.—Strain 2053 had died in stock culture before this test was carried out, and no suspension of this remained from previous tests, so that unfortunately no further examination of the strain could be carried out.

TABLE VIII.—Absorption of "1889" Serum.

Reference No. of strain	Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test coccus			
	8T	4T	3T	2T	8T	4T	3T	2T	8T	4T	3T	2T
1834	++	+	−	−	++	++	++	++	−	−	−	−
1867	++	++	++	++	−	−	−	−	−	−	−	−
Homologue 1889	++	++	++	++	−	−	−	−	−	−	−	−
1900	++	++	++	++	++	++	++	++	−	−	−	−
2026	−	−	−	−	++	++	++	++	−	−	−	−
2083	−	−	−	−	++	++	++	++	−	−	−	−
2099	++	++	++	+	++	++	++	++	−	−	−	−
2142	+	−	−	−	++	++	++	++	−	−	−	−
2172	++	++	++	+	++	++	++	++	−	−	−	−
2173	++	−	−	−	++	++	++	++	−	−	−	−
2256	+	−	−	−	++	++	++	++	−	−	−	−
2259	+	−	−	−	++	++	++	++	−	−	−	−
Control 1887 ..	++	++	+	+	++	++	++	++	−	−	−	−
2153	++	++	−	−	++	++	++	++	−	−	−	−
2156	++	++	++	+	++	++	++	++	−	−	−	−
2244	+	−	−	−	++	++	++	++	−	−	−	−

Only two strains, therefore, appear to be closely related in this series, 1867 and 1889.

An exactly similar series was then tested by means of serum prepared by inoculating strain 1900 into a rabbit, and in this instance also only two strains were found to absorb, viz., 1900 itself and 2172.

There therefore remained four strains, 2026, 2099, 2142, and 2173, where relationship to one another and to the serological types already defined had not been determined.

Reviewing the results obtained in the serological examination of this first series of fifty strains, it is seen that:—

(*a*) Thirty-seven strains are closely related to one another and constitute a fairly well defined subgroup.

(*b*) Five further strains are closely related to one another, although they exhibit no close relationship to Type I.

(*c*) Of the eight which remain, two examples each of two serological types are noted, while four strains have not been placed in relation to any of the sera so far prepared.

VIII.—Results obtained on making a Serological Examination of a Second Series of Fifty Strains.

Before discussing in detail the examination of a second series of fifty strains, I desire to place on record the results obtained on investigating, by the methods herein described, an example of that type of gonococcus which has been found to be most prevalent in London. A dried culture of this coccus was forwarded to me by Lieutenant-Colonel Gordon, C.M.G., C.B.E., of the Department of Bacteriology of St. Bartholomew's Hospital, in order that the results obtained by him in the examination of about 30

strains might be correlated with those obtained in Dundee. This correlation is of some value, for it is conceivable that the distribution of the serological types of the micro-organism under discussion might, to some extent at least, vary from one part of the country to another.

(*A*) *Examination of Gordon's Prevalent "Type" Strain.*

A culture of Gordon's prevalent type strain, preserved by drying, was suspended in saline; the suspension so obtained was standardized by the opacity method, and therewith absorption of agglutinins tests were carried out, using the same technique as that which was employed in the examination of the first series of fifty gonococci herein discussed.

The results are given in Table IX.

TABLE IX.—ABSORPTION TESTS WITH GORDON'S PREVALENT TYPE COCCUS.

	Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test coccus			
	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
	"1887" serum											
Gordon's type coccus	++	++	++	+	–	–	–	–	–	–	–	–
Homologue	++	++	++	+	–	–	–	–	–	–	–	–
	"1834" serum											
Gordon's type coccus	++	+	–	–	++	++	+	+	–	–	–	–
Homologue	++	++	++	+	–	–	–	–	–	–	–	–
	"1889" serum											
Gordon's type coccus	++	++	++	+	++	++	++	++	–	–	–	–
Homologue	++	++	++	++	–	–	–	–	–	–	–	–
	"1900" serum											
Gordon's type coccus	++	++	+	–	++	++	++	+	–	–	–	–
Homologue	++	++	++	+	–	–	–	–	–	–	–	–

These results show conclusively that only "1887" serum—provisional Type I of the series under consideration—is absorbed by the prevalent type strain encountered in London, so establishing a clear relationship between the predominant type in two widely separated parts of the country.

(*B*) *Examination by the Absorption of Agglutinins Test, of a Second Series of Fifty Strains of the Gonococcus, using* "1887"*—Provisional Type I—Agglutinating Serum.*

The technique used was exactly the same as that previously employed, but in twenty instances the test was done in duplicate, using both "1887" agglutinating serum and another Type I serum, corresponding to strain

2583—in order to exclude the possibility of the results obtained being dependent upon a peculiarity of the former.

The results obtained are shown in Table X.

TABLE X.—Absorption of Type I Serum.

	Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test coccus			
	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
	Fifteen strains reacted thus—											
A ..	++	++	++	+ or ++	–	–	–	–	–	–	–	–
	Ten strains reacted thus—											
B ..	++	++	+ or ++	–	–	–	–	–	–	–	–	–
	Four strains reacted thus—											
C ..	++	++	+ or ++	–	+ or ++	–	–	–	–	–	–	–
	Six strains reacted thus—											
D ..	++	++	–	–	+	–	–	–	–	–	–	–

The ten strains of the above, C and D, which failed to agglutinate well, were then tested to determine their absorptive capacity in respect of 1889, 1900, and in the earlier cases of the series, 1834 serum. All failed to absorb the agglutinins from these sera.

(*Note.*—Owing to a laboratory accident, 1834 serum was destroyed before the completion of the investigation.)

It may, therefore, be concluded that seventy-two per cent of cases of acute and subacute gonorrhœa in the male are caused by one fairly clearly defined serological type of the gonococcus. The figure for the first series of fifty was thirty-seven, and that for the second series thirty-five, a remarkably close correspondence.

It is worthy of note, too, that the investigation extended over the period November, 1920, to November, 1921, so that one type had remained prevalent over a period of one year.

Reviewing the results of the absorption tests as applied to these seventy-two strains, it is seen that :—

(*a*) Thirty-seven strains agglutinate to full titre in presence of, and completely or almost completely absorb the agglutinins from, Type I serum.

(*b*) Fifteen strains, while they fail to agglutinate to full titre do completely remove the agglutinins from Type I serum.

(*c*) Twenty-one strains, although they do not agglutinate to full titre, nevertheless do so markedly absorb Type I serum that they may be regarded as Type I cocci.

(*d*) One strain, although it practically fails to agglutinate in presence of Type I serum, completely absorbs the agglutinins therefrom.

(*C*) *Examination of the Remaining Fifteen Strains.*

In the examination of the first series of fifty strains, it was noted that apart from Type I (1887) serum, none of the sera prepared served to group many of the strains; it was therefore decided to immunize a rabbit with one of those strains of the second series which failed to qualify as a Type I coccus.

Strain 2453 was chosen for this purpose, and the results obtained on absorbing this serum with the fifteen strains under consideration are shown in Table XI.

In this series of tests there were also included representatives both of those strains which had been used to prepare sera for the examination of the first series of fifty and of those cocci of the first series which remained unplaced.

TABLE XI.—Absorption of "2453" Serum.

Reference No. of strain	Unabsorbed serum				Absorbed serum plus homologne				Absorbed serum plus test coccus			
	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
2286 ..	++	−	−	−	++	++	++	+	−	−	−	−
G2 ..	+	+	−	−	++	++	++	+	−	−	−	−
2450 ..	++	++	++	+	++	++	++	+	−	−	−	−
2452 ..	++	++	++	++	−	−	−	−	−	−	−	−
2453 ..	++	++	++	+	−	−	−	−	−	−	−	−
2460 ..	++	−	−	−	++	++	++	+	−	−	−	−
2465 ..	++	++	++	+	−	−	−	−	−	−	−	−
2500 ..	++	++	++	+	++	++	++	+	−	−	−	−
2536 ..	+	−	−	−	++	++	++	+	−	−	−	−
2568 ..	−	−	−	−	++	++	++	+	−	−	−	−
2581 ..	++	++	++	++	++	(+)	−	−	−	−	−	−
2603 ..	−	−	−	−	++	++	++	+	−	−	−	−
2649 ..	++	++	+	−	−	−	−	−	−	−	−	−
2708 ..	++	++	++	−	++	++	++	+	−	−	−	−
2752 ..	−	−	−	−	++	++	++	+	−	−	−	−
Cocci used for preparing sera for series No. (1).												
1887 ..	+	(+)	−	−	++	++	++	+	−	−	−	−
1889 ..	−	−	−	−	++	++	++	+	−	−	−	−
1900 ..	++	−	−	−	++	++	++	+	−	−	−	−
1834 ..	++	−	−	−	++	++	++	+	−	−	−	−
Unplaced cocci from series No. (1).												
2026 ..	−	−	−	−	++	++	++	+	−	−	−	−
2099 ..	++	++	+	−	++	−	−	−	−	−	−	−
2142* ..	..	..	..	..	..	..	..	..	..	..	..	..
2173 ..	++	++	+	−	++	(+)	−	−	−	−	−	−

* Culture 2142 dried before completion of the investigation.

Five strains of the second series are seen to correspond serologically with 2453, while two of the three available unplaced strains of the first series are found to be closely related to this strain. There therefore remain ten strains of the second series which so far have not been placed.

Using 1889 serum, it was found that only one strain absorbed the agglutinins therefrom, while in the case of 1900 serum three strains completely absorbed or markedly reduced the titre of the agglutinins.

The details of the reactions are shown in Table XII.

TABLE XII.—ABSORPTION OF "1889" SERUM.

Reference No. of strain	Unabsorbed serum				Absorbed serum plus homologue				Absorbed serum plus test coccus			
	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T	8 T	4 T	3 T	2 T
2286 ..	++	++	–	–	++	++	++	++	–	–	–	–
G 2	Neither culture nor suspension available											
2450 ..	++	++	++	+	++	++	++	+	–	–	–	–
2460 ..	++	+	–	–	++	++	++	++	–	–	–	–
2500 ..	++	++	++	+	++	–	–	–	–	–	–	–
2536 ..	++	++	++	+	++	++	+	–	–	–	–	–
2568 ..	++	++	++	+	++	++	++	++	–	–	–	–
2603 ..	++	++	++	+	++	++	++	++	–	–	–	–
2708 ..	++	++	++	–	++	++	++	++	–	–	–	–
2752 ..	++	++	++	+	++	++	++	++	–	–	–	–
Control 1889	++	++	++	++	–	–	–	–	–	–	–	–
Absorption of "1900" Serum.												
2286 ..	++	–	–	–	++	++	++	++	–	–	–	–
G 2	Neither culture nor suspension available											
2450 ..	++	–	–	–	++	++	++	++	–	–	–	–
2460 ..	++	++	–	–	++	++	++	++	–	–	–	–
2500 ..	++	++	–	–	++	++	++	++	–	–	–	–
2536 ..	++	++	++	++	++	–	–	–	–	–	–	–
2568 ..	++	++	+	–	++	++	++	++	–	–	–	–
2603 ..	++	++	+	–	++	(+)	–	–	–	–	–	–
2708 ..	++	++	+	–	++	(+)	–	–	–	–	–	–
2752 ..	–	–	–	–	++	++	++	++	–	–	–	–
Control 1900	++	++	++	++	–	–	–	–	–	–	–	–

It is, therefore, seen from this table that one strain absorbs 1889 serum and that three absorb 1900 serum.

Unfortunately, owing to a laboratory accident, 1834 serum was accidentally destroyed before the remaining unplaced strains—2752, 2568, 2460, 2450, and 2286—had been investigated, while strain G 2 died before the completion of the tests.

SUMMARY OF RESULTS.

Type	Series I	Series II	Total
Type I—1887 ..	37 ..	35 ..	72
2453 ..	2 ..	5 ..	7
1889 ..	2 ..	1 ..	3
1900 ..	2 ..	3 ..	5
1834 ..	5 ..	Not available ..	5
Unplaced	2 ..	6 ..	8

The thanks of the author are heartily expressed to all those who took a kindly and stimulating interest in this work, but special thanks are due to Dr. W. A. Alexander, Dr. G. R. Ross, and Mr. Small, of this laboratory, whose perseverance and assistance really made possible the completion of the investigation.

REFERENCES.

[1] Arkwright. *Journ. Path. and Bacteriol.*, 1921, xxiv, pp. 36-60.
[2] Besredka. *Ann. de l'Inst. Past.*, 1901, xvi, p. 918.
[3] Bordet (Collected Studies Gay). "Studies in Immunity," Wiley and Son, New York, 1909, p. 527.
[4] Bordet and Ciuca. *Compt. Rend. de la Soc. de Biol.*, 1920, lxxxiii, p. 1293.
[5] Castellani. *Zeitschr. f. Hyg.*, 1902, xl, pp. 1-20.
[6] Cole, S. W., and Lloyd, D. J. *Journ. Path. and Bacteriol.*, 1916-17, xxi, pp. 267, 286.
[7] d'Herelle. *Compt. Rend. de l'Academie de Sc.*, 1917, clxv, p. 373; *ibid.*, 1918, clxvii, p. 970; *ibid.*, 1919, clxviii, p. 631.
[8] Douglas, S. R. *Brit. Journ. Exp. Path.*, 1921, ii, pp. 175-191.
[9] Gordon, M. H., and Hine, T. G. M. Med. Res. Committee, Special Report Series, 1918, No. 19, pp. 23 and 24.
[10] Hermanies. *Journ. Infect. Dis.*, Chicago, 1921, xxviii, p. 133.
[11] Jötten. *Munch. Med. Woch.*, 1920, lxvii, p. 1067.
[12] Kolmer, J. A. "Practical Text-book of Infection, Immunity and Specific Therapy," Saunders, 1917, p. 503.
[13] Martin, W. Blair. *Journ. Path. and Bacteriol.*, 1911, xv, pp. 76-106.
[14] Nicolle, Jouan, and Debains. *Ann. de l'Inst. Past.*, 1919, xxxiii, p. 261.
[15] Teague and Torrey. *Journ. Med. Res.*, 1907-8, xvii, p. 223.
[16] Thjötta and Avery. *Journ. Exp. Med.*, New York, 1921, xxxiii, p. 763, and *ibid.*, xxxiv, p. 97.
[17] Thomson. Med. Res. Committee, Special Report Series, No. 19, pp. 18-20.
[18] Torrey. *Journ. Med. Res.*, 1907, xvi, p. 329, *ibid.*; 1908, xix, p. 471.
[19] Warren, S. H. *Journ. Path. and Bacteriol.*, 1921, xxiv, p. 424.
[20] Watabiki. *Journ. Infec. Dis.*, 1910, vii, p. 159.
[21] Wollstein. *Journ. Exp. Med.*, New York, 1907, ix, p. 588.
[22] Swartz. *Journ. Urology*, Baltimore, 1920, iv, No. 4, pp. 325-345.

ON THE INSTINCTIVE FACTOR IN HYSTERIA.

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Instinctive Behaviour in General.

" Instinct is a great matter."—*Falstaff.*

(*Continued from p.* 37.)

Hysteria.

I have endeavoured in the preceding remarks to indicate some of the springs of action in animals and in man; and we have noticed that it is by no means easy to draw the lines between reflex, instinctive, intelligent and rational actions. The rôle of the instincts of self-display and of self-abasement (the self-regarding instincts) has also been emphasized in regard to human conduct. Upon these considerations it seems possible to develop a satisfactory theory of " hysteria " in certain of its aspects.

Some of the phenomena to be explained are: (*a*) Sensory, such as localized anæsthetic areas of the body, paræsthesiæ, defects of special senses; (*b*) motor, such as hemi- or monoplegia, aphonia and dysarthria, contractures, spastic phenomena, tremblings, epileptiform attacks; (*c*) disturbances of reflex actions such as that of micturition; (*d*) digestive—such as gastric pain, loss of appetite, borborygmi, diarrhœa; (*e*) respiratory, such as asthma, "hay-fever," sneezing, coughing and hiccough; (*f*) circulatory—such as palpitations, syncope, pseudo-angina and vaso-motor disturbances. That these manifold symptoms shall be considered hysterical it is essential that they shall be devoid of any basis of structural change in the organs concerned; that they shall be removable, at any rate temporarily, by some form or other of persuasion; and that they shall not be produced by the conscious, purposive action of the patient.

The first of these criteria is obviously not easy to establish in all cases for anatomical reasons. Even the most skilful physician may have been puzzled at times to say whether an early case of disseminated sclerosis, for example, was organic or functional; and the problem is further complicated by the frequent addition of hysterical phenomena to structural disease. Fortunately, however, many of the manifestations of hysteria are incompatible with a diagnosis of lesions of the great systems, as "stocking anæsthesia" is with any possible spinal lesion.

The second diagnostic method is the most reliable, and is specially favoured by Babinski [30] who has proposed calling hysteria pithiatism in consequence of its being amenable to persuasion. If symptoms like paralysis can be seen to disappear under any form of persuasion from gentle conversation to the brutal application of painful stimuli, then it is of course quite certain that the disease cannot be due to gross structural change.

The third criterion is the most difficult of all to apply, for the reason that the conscious and unconscious activities of any organism are exceedingly difficult to separate from each other. The fact that several names, unconscious, subconscious, co-conscious [31], unwitting [32], are in use shows that either the ideas or terminology of the subject are not yet completely clear. Nevertheless, in spite of this we have, when confronted by a case of supposed hysteria, to make as sure as we can that the patient is not carrying out a pre-arranged and calculated plan for outwitting the doctor to gain his own ends. The only things to depend on are a very careful physical examination which may disclose contradictory symptoms, and the employment of such common sense and knowledge of human nature as we may be able to command. In the cases to be quoted here it should be taken as stated that the diagnosis was arrived at by the application of these principles as far as circumstances allowed.

The following belongs to a common class of case. A girl of about 18 years of age was seized at night with dyspnœa. Her parents were greatly alarmed and thought she was going to die. I saw her soon after the onset of the trouble. The room was full of anxious relatives, and the patient was sitting up in bed breathing rapidly, and with slight laryngeal noises. She seemed in terror of suffocation and moved about restlessly. Her complexion was rosy. The respiration resembled that of a runner rather than that of an asthmatic. No trouble could be discovered in the upper air passages. A complete physical examination of the chest did not disclose any disease. The treatment adopted was to empty the room of everybody except one old woman. The patient was then reassured and induced to lie still, and in a short time she was asleep for the night, breathing normally. Inquiry elicited that she had quarrelled with her sweetheart on the day preceding the attack and had, of course, been brooding over the event when the illness began. The points about this case were the patient's great distress, real fear (as far as could be judged) and the relief that was afforded by a thorough physical examination and a few reassuring words, together with the banishment of spectators from the apartment.

Another case, also of a common type except for the age of the patient, was as follows: A man aged between 60 and 70 sent an urgent message for assistance on account of "strangulated hernia." I found him lying on the floor, wearing a suspensory bandage and a double inguinal truss. He seemed to be terrified and in pain, and stated that in addition to the strangulation he suffered from varicocele. A complete examination revealed no sign of any kind of disease. It was with great difficulty that he could be induced to believe that he was not ill, but eventually he accepted the new idea. His history was that he had been afflicted with varicocele and hernia, which "often became strangulated" ever since the death of his son in action about a year previously.

A third case is that of a school boy who, after a "ragging" episode of which he was the victim, exhibited from time to time attacks of spasmodic

rigidity of the limbs and body during which he talked a good deal, giving vent to nonsensical "patter" such as is heard in music hall "turns." The seizures varied in duration from a few minutes to several hours, and were not remembered by the patient. Removal from school combined with open-air employment rapidly effected a cure; but he had later a slight relapse following an unfortunate love affair.

These cases illustrate very well the occurrence of hysterical symptoms preceded by injury to the patient's *amour propre*, in other words by a stimulus to the instinct of self-abasement arousing an emotion which became intolerable. The girl had been depressed by her lover's hard and perhaps unjust remarks; the old man, his personal ambition long dead, had been baulked in his second ambitions entertained on behalf of his son; the school boy had to face the gibes of his companions, and was made to feel acutely that he was an inferior person in the estimation of the savage little world of school. All three suffered the same emotion of subjection; and for all three the busy coming and going of doctors, the hushed voices of the household and the anxious inquiries of friends afforded the same consolation by rousing and satisfying the instinct of self-assertion. In the case of the girl the emotion of subjection or negative self-feeling was evoked by failure in love, or in other words by a blow to the reproductive instinct; in the case of the old man it was evoked by a blow to the parental instinct; and in the case of the boy by fear.

That such injuries to self-esteem can produce disordered conduct is, of course, no new idea. In 1887 Bristow quoted Sir Thomas Watson as saying when lecturing on hysteria: "Behind the moody, reserved and tricky behaviour there often lies some mental or emotional cause—some hope deferred or disappointed—which being ascertained and capable of satisfaction and satisfied, the patient may be restored to her customary health" [33].

Simple physical pain is not so often an excitant of hysterical manifestations as is "mental trauma." Possibly this is because physical pain does not as a rule last so long or act so continuously. Nevertheless, cases with this origin are often observed. Examples are the following: (*a*) A middle-aged lady of highly sensitive temperament knocked her shin violently, by accident, against some hard object. A bruise and small abrasion resulted. Following this the leg became paralysed, and the patient could only get about on crutches or in a wheeled chair. The leg wasted considerably, and, though not anæsthetic, resembled a paralysed limb. She came under my care two or three months after the injury. A careful examination did not reveal any disease, and under suitable moral treatment she rapidly recovered and took to playing golf. (*b*) A young girl had to undergo a slight operation for the removal of a sebaceous cyst. This was done under imperfect analgesia. She began to suffer shortly afterwards from anorexia nervosa which persisted for some years to the wonder and distress of her household. (*c*) The cases sometimes seen in war, and

especially well described by Pitres and Laffaille [34] of paralysis of the hand following an uncomplicated flesh wound of the arm. All these cases can be explained in the same way as the previous ones if we remember that bodily pain is one of the excitants of the instinct of self-abasement.

Occasionally the instinct of self-assertion, without any traceable previous depression, is responsible for hysterical symptoms; but such symptoms are merely due to an unnaturally strong desire to be in the "centre of the picture" and have not, as a rule, the emotional force that accompanies the phenomena arising from an intolerable feeling of self-abasement; and hysteria which cannot be traced to an antecedent depression is not of a very intractable or severe type. It mostly occurs in children, who dearly love to be the focus of attention of their elders. Hector Cameron has especially drawn attention to this type [35] and mentions a boy who called out the word "Stomachs" for many days with almost insane persistence merely to keep alive the excited interest of his parents. The following case is perhaps more definitely hysterical in type. The parents of a little boy were very anxious that he should grow more quickly, and consequently meal times became given over to exhortations to him to eat. He soon grew aware of the fact that when his appetite was poor he was a person of great importance in the household, and the final result was that he became the victim of a very marked anorexia nervosa which persisted till he went to a boarding school. Nobody in this institution cared much whether he had an appetite or not, and a permanent cure was soon established without any special treatment.

Of the primitive human preoccupations in which failure is apt to produce hysterical symptoms, none is more important than the business of reproduction. The very word hysteria indicates the frequency with which disorders of reproduction are accompanied by disturbance of the functions of other systems of the body. The explanation of this probably lies in two facts: one, the importance and imperiousness of the instinct, and the other the close association between it and the instincts of self-display and self-abasement. A pompous, assertive manner is characteristic of the primitive male lover, and a submissive, self-effacing demeanour is characteristic of the female. It is here suggested that as the instinct of subjection is more highly developed in the woman than in the man, so the painful excess of its emotion, negative self-feeling, will more frequently occur; and consequently the phenomena of hysteria will be mainly found in the female in ordinary life.

It is not surprising that until recently the reproductive instinct should have been held to be associated with the majority of hysterical cases seen, for it and the self-regarding instincts are the only ones that hold extensive sway among civilized populations: and the self-regarding instincts have been inexplicably neglected by physicians. But with the advent of the war another equally powerful impulse found extended scope, viz., the instinct of flight or self-preservation with its affect or emotion of fear. After

hostilities had begun it was promptly recognized that fear was a first-class agent in the production of functional disease.

One of the first cases that came under my care was the following: An officer of robust build was subjected to the usual stresses undergone in the forward area. He bore it patiently for a time and then began to exhibit a condition of spasmodic torticollis. This was worse when he was talking to anybody, and though it was quiescent when he was alone, it started again the moment anybody came into the room. Treatment in the zone of war proving useless he was sent to the base and to England. At home he gradually became cured under the influence of pleasant surroundings and was eventually sent back to the front. After a short stay amongst the scenes of battle the disease reappeared the same as before and he was sent home again, and it was unfortunately impossible to trace his later history. Many similar cases came under my care. The most common symptoms were paralyses, tics, spasms, aphasia, dysarthria, etc., which necessitated the prompt despatch of the sufferer to the base. Sometimes the symptoms came on directly the first shell burst, sometimes at the end of a long exposure to danger and hardship. An interesting feature, often reported, and to which Babinski attached much importance [36] is that the symptoms frequently come on after an interval of "incubation." I found that this was true as a rule when the disability prevented locomotion, but not necessarily in cases such as mutism where the patient's powers of getting about were unaffected. Two good illustrations are afforded by the following notes: Second Lieutenant Y, aged 22, one month in France, all the time in the line. After being with a working party which was employed in a dangerous place he returned to a dug-out and was all right when he arrived. He slept till morning when he discovered he could not use his left arm and leg. He told the doctor he could not move and was sent to hospital on a stretcher. In hospital he stated that a year ago he had been in hospital for a month with "nerves" following a bomb explosion (accidental). He could give no details of his "nerves" except that he slept badly and was shaky in his legs. On examination the left arm and leg were flaccid and no voluntary movements could be carried out with them. No anæsthesia was present. Tendon reflexes were normal. The orbicularis oris and platysma on the left side were twitching continually. After a few days rest in hospital (during which the twitching of face and neck ceased) he was anæsthetized with ether which was only pushed far enough to produce excitability and struggling. He yelled words of command at the top of his voice and struggled violently with his unparalysed limbs. He was allowed to come out of the anæsthetic and told to move his arm and leg, but he would not do so until I re-applied the mask, when he began to move them after a few inhalations. He was made to walk immediately after this and in two days was perfectly well and taking long country walks.

The second case is that of Lieutenant G, aged 34. In the front line

for three months. Went into an exceptionally bad place two days before admission to hospital. Saw the infantry on his left falling back and was very scared. Kept his machine gun in a shell-hole all day and was shelled the whole time, so that he had more than once to move to another hole as the ones he was in were blown in. This state of affairs continued through the night. In the end he became so dazed that he had not the sense to move even when a safer place was available. Was relieved at 3 a.m. after having eleven casualties out of the twenty-five men in his squad. Three were killed. When relieved he walked back through heavily shelled areas. On reaching reserve trenches his commanding officer ordered him to the transport lines to get a good breakfast. He remembers nothing after having breakfast. Does not remember being admitted to hospital. State on admission : Right arm and leg powerless. Irregular patches of analgesia to pin-prick in paralysed limbs—quite deep stabs produced no sign of pain. Tendon reflexes of affected limbs much diminished. No other abnormal physical signs. Mentally very irritable. Recovery occurred in a couple of days under persuasion.

On first considering this class of cases they seemed to me to be attributable to the same instincts as the hysterical phenomena of civil life—an intolerable excitation of the instinct of self-abasement followed by conduct which would minister to the instinct of self-display and produce its emotion of positive self-feeling. It seemed that in the awakening of terror by sights and sounds that one's comrades were able to face better than oneself, there was ample cause for the production of the deepest degrees of negative self-feeling. But further observation suggested that though this might be true in some cases, in many the explanation was not applicable. In the first place, long association with officers collected in a "shell-shock" centre led irresistibly to the belief that not a few of them differed from their comrades who remained in the trenches in being to a great extent impervious to shame. The fact that they could not face gunfire did not seem to them to be extraordinary or a cause of depression any more than a dislike of milk or eggs (for example) seems shameful to those who have the dislike.

Secondly, if they did feel ashamed, their *amour propre* could not receive any satisfaction when they "went sick." The regimental commanders considered such officers a nuisance and a danger and did not scruple to tell them so; the men serving under them, while not despising honest fear, openly sneered at an officer who went sick with nervous troubles; and when the "shell-shocked" got into hospital he found himself one of a crowd and of no particular interest to anybody. In short, it became in time a blot on a man's record if he had shell-shock. I was, therefore, driven to the conclusion that the functional nervous diseases seen in war were in most cases developed directly in satisfaction of the instinct of flight. The appearance of aphonia or paralysis, for example, ensured that the sufferer would be rapidly evacuated from the firing-line to the compara-

tive safety of a hospital. The fact that disorders of locomotion did not come on until the danger was passed, pointed to the utilitarian nature of the symptoms, for if a man became unable to move when in the danger zone his danger would be greatly increased, but if the symptoms developed later they would save him from a repetition of the risks. It is not suggested that these men consciously simulated disease in order to escape danger; for when soldiers set out to render themselves ill with a forethought purpose of being evacuated, they wound their hands or feet, put irritating substances in their eyes or inject caustics into their muscles, etc. What is proposed is that the "hysterical" phenomena arising in the firing-line constitute an unreasoned obedience to the imperious instinct of flight, whereas self-inflicted wounds and conditions such as dermatitis artefacta illustrate the same instinct employing reason for its satisfaction. With the former may be classed Fabre's insects preparing palaces and larders for progeny they will never see, or the young human female beautifying herself and seeking the society of men before she understands the full meaning of sex; whereas the latter is equivalent to the conduct of the man who dams the stream in the knowledge of the coming drought of summer.

We have already seen how infinitely resourceful the living organism is in satisfying the impulses of blind instinct, and we have also seen how hard it is to say exactly where that blindness begins to be illuminated by the first glimmer of intelligence and reason. It is surely no more and no less wonderful that functional disorders should arise in man under the influence of an instinct than that a beetle should sham death when frightened or a bird pretend to be wounded under the influence of parental instinct. Whatever the explanation of the instinctive reactions of the lower animals may be that also is the explanation of hysteria in man, and is permeated with the same difficulties of allotting the proper values to conscious and unconscious conation in any given case.

It has been objected, as an example of these difficulties, that such a condition as cutaneous anæsthesia cannot be considered as under the control of the mind; yet Hurst [37] mentions a case where a man who was anæsthetic in his feet when awake could, when asleep, be awakened by a touch on the anæsthetic area. More formidable objections can be put forward in regard to such phenomena as palpitation of the heart, borborygmi, dermographia, and similar vasomotor troubles; and it seems difficult to believe that such symptoms can be the attempt of the subject to satisfy an instinct. In this connexion we should begin by remembering that the diagnosis of hysteria has, in the past, been sometimes rather light-heartedly made, and too great reliance has been placed on the stigmata. In fact, Babinski, after an exhaustive research into such cases, disbelieves entirely in the hysterical origin of trophic and circulatory disturbances. As he quaintly expresses it, the deductions from observations made on these conditions have been "vaines, entachées de nullité" [38]. But, if

this opinion be held to represent merely the extreme of the reaction against the old school of the Salpêtrière, the occurrence of such symptoms need not be held as inexplicable on the theory of instinctive response. Considering, for example, the almost uncanny gift of the omentum of inserting itself into dangerous lacunæ in the abdomen to keep out the more vital viscera, or its power of enveloping and limiting the extension of infective foci; or, again, considering the hypertrophy of the kidney or testicle whose fellow has been removed or the complicated adjustments occurring in the repair of bone, or the increase of red blood-cells that occurs when oxygen is lessened in the air breathed, or the efforts of the tissues to encyst or extrude a foreign body, or the extraordinary complication of reactions required to vomit an irritant from the stomach—considering all these efforts of the unconscious tissues in defence of the life of the body, we are surely not justified in denying to the lower centres of the brain the power of modifying vasomotor, trophic, or peristaltic conditions in furtherance of the purposes of a sufficiently powerful instinct, working as all instincts do, mainly below the level of conscious thought.

(*To be continued.*)

MITES ON MOSQUITOES.

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In his interesting Entomological Notes in the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS for June, 1922, Major J. E. M. Boyd, referring to the incidence of mites on mosquitoes, mentions the work of Major Langrishe which he says proved that these larval mites are actual parasites of the mosquitoes, and states that he has determined them to be species of Hydrachnidæ (water mites). He regards the subject as one of considerable interest and solicits information from members of the Corps stationed abroad.

As some of the latter may take up the matter, and as Major Boyd does not consider the literature on the subject, it has occurred to me that it may be useful if I give a brief résumé of the latter. I cannot undertake to ensure that it will be complete, for time does not permit of a very thorough search, hence I will only refer to those papers regarding which I have references and amongst which is what I believe to be the chief pronouncement on the subject.

The question of mosquito parasites is one in which I have always been interested since, as recorded in the First Report of the Wellcome Research Laboratories, Khartoum, I found in 1903 what I believed to be two species of mites parasitic on mosquitoes in the Sudan—notably on *Mansonia uniformis* and *Myzorhynchus* (*Anopheles*) *paludis*. In those days I spoke of them as tiny ticks and distinguished red and green varieties. They were found on various parts of the mosquito's body, even, it may be noted, on the wings, but as a rule were attached to the thorax or abdomen. I made no attempt to study them in detail but forwarded them to Mr. F. V. Theobald. Apparently, however, he did not investigate them, as they are not mentioned in his great work on the Culicidæ.

I had occasion again to refer to the matter in the First Review Supplement of the Khartoum Laboratories when reviewing the "Report of the New Jersey State Agricultural Experimental Station on Mosquitoes," 1904, by J. B. Smith. This author had a very interesting article on the natural enemies of mosquitoes and referred to the presence of parasitic red mites, which it was said serve to weaken the insect and possibly to shorten its life. Smith, however, admitted that little was known with certainty regarding the life-history of these minute ecto-parasites.

With these preliminary remarks we may proceed to a brief review of the available literature.

There seems to be no doubt that Grassi was the first to note the presence of ecto-parasites on mosquitoes. He merely refers to the matter in a footnote to one of his papers published in 1900, stating that he had found an acarus on the imagines of *A. claviger* (*maculipennis*).

Another very early reference is that of Fearnside (1900) who, writing

from India, described, with an illustration, vermilion-coloured mites on what he called *Culex pipiens* (probably *C. fatigans*). He describes them as oval or round and gives their measurements. Those attached to the thorax were grey, those on the abdomen red, and the red colour is said to be due to the hæmoglobin of the blood in the stomach of the mosquito. He believed that the mandibles penetrate between the somites and reach the insect's stomach.

Hodges, in 1902, writing from Uganda, refers to the infestation as follows :—

"An interesting point about these Panoplites is that they were found to be infested in considerable numbers with a minute parasite resembling a tick. In some cases quite 50 per cent were attacked, and as many as nine parasites have been found on one mosquito. The same, or a very similar, parasite was also found in two instances on *A. paludis* (Theobald).

"The acarus itself (see drawing, fig. 2) is sometimes whitish or grey but generally orange red, the colour depending probably on the food of its host, as on a gorged mosquito it is always red. It seems to attach itself by preference to the junctions of the thorax and abdomen or of the thorax and head, but also frequently along the lines of abdominal segmentation. It appears to affect the health of its host, which is generally sluggish and does not live many days in captivity. The acarus taken from the bodies of filaria-fed mosquitoes was examined for *F. perstans* with negative results."

As indicated, he gives a drawing of the mite and also a sketch of the "under-side of its head," but he does not go more deeply into the question.

In the same year Laveran mentioned that he had found acari on different species of Culex and Anopheles sent him from Tonkin and Madagascar. He submitted them to his colleague, Dr. Trouessart, who reported that those present in the alcohol which had contained the mosquitoes from Tonkin were *Tyroglyphus siro* (L.), *Chryletus eruditus* (Schrank), and the nymph of an undetermined species of *Gamasus*. According to Trouessart these were not true parasites but merely adventitious organisms. However, he found hexapod larvæ of true water-mites fixed upon a Culex from Tonkin, and though they were too young to enable him to determine them, he suggested that on account of their greenish colour they were probably the larvæ of *Arrhenurus*. Another hexapod larva attached to an Anopheles from Madagascar he considered as probably belonging to *Hydroma* or *Nesœa*. Laveran notes that Macdonald had observed mites on mosquitoes at Rio Tinto.

Giles, in the second edition of his handbook (1902), refers to miniature acari which W. J. Cropper had found on *A. maculipennis* in Palestine and of which he had sent him a preparation.

Giles says: "They are of cordate outline, nearly as wide as they are long, and are provided with a formidable suctorial mouth. From their comparatively large size, they must be formidable parasites to so small an insect, but only three specimens were observed to be infested in this way. They much resemble the hexapod larvæ of certain bird-ticks."

In the following year (1904) a paper on the subject of the mites found on mosquitoes was published by Gros. He found them in Algeria, both on anophelines and on a species of culex, and described them in some detail. Those with which he dealt were of a red colour, and he noted that the colouring matter expressed by compression of the mite during examination diffused itself throughout the xylol which he employed.

Shortly afterwards another paper on the mites found on anophelines by Edmond and Etienne Sergent made its appearance. It embodied observations they had made in Algeria on these parasites since 1900. They had found the acari only on anophelines, never on culex or any other species of mosquito.

Their material had been submitted to Trouessart, who found the mites to be the hexapod larvæ of Hydrachnids and thought they might possibly appertain to the genera Eylais, Hydrodroma, Hydryphantes, or Diplodontus.

The Sergents, in the case of *A. maculipennis*, noted an infestation by the mite at all stages of the mosquito larvæ, nymphs and adults, and state that the parasites did not appear to incommode their hosts. They were able to watch the development of the parasitized larvæ, which followed the normal course, and they observed that the Hydrachnids passed from stage to stage of the developing mosquito.

A single infestation was found to be rare. As many as ten mites were seen on the same insect. In the case of the adult mosquito they were attached to the abdomen, in that of the nymph to the dorsal surface near the spot where the pupal membrane splits. It was found by experiments that the mites could change their hosts, *A. algeriensis* being infected from *A. maculipennis*, and the view was expressed that the parasites are unlikely to be in any way serviceable in anti-mosquito work.

The paper by Smith in the same year has already been cited.

In 1905 Blanchard's "Les Moustiques" appeared and contained a short note on the subject. He mentions meeting with infestation on three occasions, once in an *A. maculipennis* from Bastia in Corsica, once in *A. bifurcatus* from Charbonnières, and once on an undetermined anopheline from Madagascar. He thought the mites might be the larvæ of *Nesœa fuscata*.

Of some interest is a note to the effect that at Madeline Island, Wisconsin, U.S.A., the mosquitoes are frequently attacked by small red mites which fix themselves to the wings and weaken their hosts. These acari suggest the larvæ of Trombidiidæ rather than those of Hydrachnidæ and may gain access to the mosquito while it rests on plants.

It might seem from this observation, from that of Trouessart, already mentioned, and from what I myself noted as regards wing infestation that two distinct types of acari may infest mosquitoes: one a land animal, the other with its habitat in the water; but this apparently is not the case (*vide infra*).

Dyé's paper (1905) on "Les Parasites des Culicides" devotes consider-

able space to the mites of mosquitoes. The literature is passed in review, not only as regards the question of mosquito infestation but also with reference to the general study of Hydrachnidæ.

In view of Major Boyd's requirements it may be well to transcribe some notes by Dyé on the technique for the preservation of the mites. He recommends clearing the acari either by boiling for a few seconds in a weak solution of potash (a procedure requiring some experience) or by the application of cedar oil or the lactophenol of Amann. The clearing process should be watched under the microscope. The mites may then be mounted in glycerinated gelatine, pure glycerine or Canada balsam.

Dyé studied the mites of mosquitoes sent from Madagascar. The latter included *A. costalis*, *A. coustani* and *Mansonia uniformis* var. *africana*. Drawings are given showing the hexapod larva both free and attached to the head of a *Mansonia*. The author found it impossible to determine the acari, but he divided them into three types, giving details which it is scarcely necessary to mention here.

Howard, Dyar and Knab (1913) in the "Mosquitoes of North America" devote three pages to the subject, chiefly in the way of a review of the literature. They deal a little more fully with the findings in Wisconsin and comment upon them. I quote this part of their paper.

"One of us (Howard) has quoted a letter from Mr. E. P. Salmon, of Beloit, Wisconsin, in which it was stated that one of those mites attacks the mosquitoes of Madeline Island a few weeks after their appearance in June, and that from the time the little red creature appears under the mosquito's wings the latter begins to lose its strength. 'After a few weeks, along toward the end of July, the mosquito ceases to be very troublesome and seems to be fighting with his parasite for his life.' The writer stated that the parasite is probably one of the red mites found upon flies and particularly upon the common house fly. Mosquitoes being aquatic insects, it was suggested that the mite observed by Mr. Salmon might be one of the little water mites of the family Hydrachnidæ, but that the mosquito issues from its pupa so rapidly that this was hardly likely to be the case. It was suggested that it might be one of the Trombidiidæ, the young of which may crawl upon the mosquito when it is at rest upon the plants (Howard's "Mosquitoes," pp. 165-166).

"Since that time a number of these mites have been sent in and others have been examined by other observers. All have been found to be hydrachnids, and so far as known to us there is no exact record of the finding of a Trombidiid on a mosquito. Specimens of these mites taken from a mosquito at Kanawha Station, W.Va., by Dr. A. D. Hopkins, have been determined by Mr. Nathan Banks as larvæ of the hydrachnid genus *Eylais*. Larvæ of an *Eylais* have also been determined by Mr. Banks upon mosquitoes from the Philippine Islands sent in by Dr. Clara S. Ludlow. Other larval hydrachnids found upon mosquitoes have been sent to us from the Great Slave Lake by Ernest Thompson Seton. We have

ourselves frequently found them, and it may be stated, in a general way, that their presence on mosquitoes is a common occurrence."

They reproduce a statement by Mr. James K. Thibault, Jr., of Scott, Arkansas, which is very interesting and may be given here, especially as it refers to mites of a bluish-green colour that may be identical with the one which I found on *Mansonia uniformis*.

Mr. Thibault writes as follows: "More or less every adult specimen of certain species is found to be infested by a red mite very similar in general appearance to those occasionally found on house flies, though for the most part such will be the case only early in the season—spring. The proportion of infested individuals among the various species is very striking, *Anopheles* and *Mansonia* showing the greatest number of infested individuals, likewise the greatest number o mites per mosquito. The first *Anopheles* and *Mansonia* to hatch out show over ninety-five per cent of infested specimens, and indeed it is well-nigh impossible to find a single specimen that is not parasitized. *Culex abominator* D. and K. probably comes next, but in this case the mite is bluish-green—about the same shade as that seen in *Culex abominator* itself when first hatched. This probably accounts for the blue-green colour of the otherwise red mite.

"I list the various species which I have found to be infested, placing those showing the greatest percentage of infested individuals first in the list, and so on in order:—

"*Anopheles crucians* Wied; *Anopheles quadrimaculatus* Say; *Anopheles punctipennis* Say; *Mansonia perturbans*, Walk.; *Culex abominator* D. and K.; *Culex territans* Walk. (rarely); *Aëdes triseriatus* Say (only once); *Megarhinus septentrionalis* D. and K. (rarely).

"Nearly all of the above-named species hibernate as adults, and one might be led to believe that there was some connexion between this and the fact that they are so often parasitized, but I do not think this is the case. I believe that it is more probable that the mites attach themselves to the mosquitoes when the latter are in the larval or pupal stage, perhaps both. I once found three larvæ of *C. abominator* all having mites upon them. These mites were of the typical *red* colour, whereas those found upon adult *C. abominator* are invariably bluish-green. This difference in colour is, I believe, to be explained by the fact that the larvæ of this mosquito are not green, even when found among vegetation of that colour, until nearly full grown, after which, like the larvæ and pupæ of *A. quadrimaculatus*, they may be bright grass-green. These mites lived for several days on the larvæ, but when removed from them and placed in water, they soon sank to the bottom and died.

"It is possible that the bluish-green mite of *C. abominator* is a different species, for I have never found them of this colour on *A. quadrimaculatus*; yet under similar circumstances the larvæ, pupæ and freshly emerged adults of this mosquito are also bright green. It is certainly very probable that this mite destroys quite a good many mosquitoes, and it is unfor-

tunate that they do not extend their operations throughout the season instead of only the early part.

"A good many species seem to be for the most part never or only very rarely attacked at all."

In the light of these observations it is unfortunate that I have no notes regarding the green mite met with in the Sudan. I cannot remember definitely if it occurred by itself on the mosquito, but rather think I found it in association with the red variety.

According to Banks, who worked in the vicinity of Washington, mite-infested mosquitoes do not appear to suffer in any way. He says the mites cannot live long out of water and can never complete their transformations. If very numerous it is conceivable that their weight might hinder the mosquito in its flight or they might so drain it as to seriously weaken it.

An example of heavy infestation is given by Major Boyd himself in an earlier paper than that of which this review is the outcome. He cites a case of a female *A. maculipennis* with thirty-three arachnid passengers. He made sure of their being hydrachnids by drowning infested mosquitoes in water, when the larval mites abandoned their dead hosts and in a few days underwent development.

It is clear that these ecto-parasites of mosquitoes still require attention and study, and it is hoped this short compendium of the literature regarding them will be helpful to any who may decide to accept Major Boyd's invitation.

REFERENCES.

BALFOUR, A. (1904). "Mosquito Work in Khartoum," *First Report Wellcome Research Laboratories at the Gordon Memorial College, Khartoum*, p. 35.

BALFOUR, A., and ARCHIBALD, R. G. (1908). Review of some of the recent advances in tropical medicine, *Supplement to Third Report Wellcome Research Laboratories, Khartoum*, p. 132.

BLANCHARD, R. (1905). "Les Moustiques," p. 134.

BOYD, J. E. M. (January, 1922). "The Botany and Natural History of the Dyke-land," JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, p. 44.

Idem. (June, 1922). "Entomological Notes," p. 459.

DYÉ, L. (1905). "Les parasites des Culicides," *Arch. de Parasitol.*, p. 60.

FEARNSIDE, C. J. (1900). "Parasites found on Mosquitoes," *Indian Medical Gazette*, vol. xxxv, p. 129.

GILES, G. M. (1902). "Gnats or Mosquitoes," 2nd edition, p. 151.

GRASSI, B. (1900). "Studi di uno zoologo sulla malaria," Rome, p. 153.

GROS, M. H. (January, 1904). "Sur un Acarien parasite des *Anopheles*," *Compt. rend. de la Soc. de Biol.*, vol. lvi, 1er semestre, p. 56.

HODGES, A. (October 1, 1902). "Sleeping Sickness and *Filaria perstans* in Busoga and its Neighbourhood, Uganda Protectorate," *Journ. Trop. Med.*, vol. v, p. 300.

HOWARD, DYAR and KNAB (1913). "The Mosquitoes of North America," vol. i, pp. 172-174.

LAVERAN, M. A. (March, 1902). "De quelques parasites des Culicides," *Compt. rend. de la Soc. de Biol.*, vol. liv, p. 233.

SERGENT, EDMOND and ÉTIENNE (January, 1904). "Note sur les Acariens parasites des Anopheles," *Compt. rend. de la Soc. de Biol.*, vol. lvi, p. 100.

SMITH, JOHN B. (1904). *Report to the New Jersey Agricultural Experimental Station upon the Mosquitoes occurring within the State, their habits, life history, etc.*, p. 81.

COMPARISON OF THE WASSERMANN REACTION WITH THE SACHS-GEORGI REACTION AND WITH THE SIGMA REACTION OF DREYER AND WARD.

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AND
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Flocculation Reactions in Syphilis.

Much work is now being done on the flocculation tests for syphilis; the reagents required are fewer than those required for the Wassermann test, and, as the results are accurate, it is not improbable that some form of flocculation test may soon largely, if not completely, replace the Wassermann test. The chief advantage of flocculation tests is that the only reagents required are an antigen, prepared from calf or horse heart, cholesterin and saline solution, while, for the Wassermann reaction, similar reagents are required and, in addition, sheep's red blood corpuscles, guinea-pig serum and amboceptor.

A considerable number of methods of performing flocculation tests have been described, the best known being that of Sachs and Georgi and of Meinicke. Verne's test is most used in France, and in Britain Dreyer and Ward's method is best known. These tests are all the same in principle. Early in 1921 we performed 246 Sachs-Georgi tests in this laboratory, with fairly satisfactory results, although our confidence in the test was considerably shaken by our getting a negative result with seven sera which were clinically secondary syphilis and gave strong positive Wassermann results. At that time we put up the test with only one dilution of serum and antigen, and it is now known that, on account of inhibition zones, positives may be thus missed, but if several dilutions are put up better results are obtained. We have now begun a new series of tests with three dilutions of serum 1 in 5, 1 in 10, and 1 in 20, also we now place the tubes in a water bath at 37° C., instead of in the incubator, and take readings at four hours in addition to the twenty-four hour reading. So far, the results are very good.

The table on next page is a summary of our first series of Sachs-Georgi tests.

Dreyer and Ward's Method of Performing a Flocculation Test called the Sigma Test.

Our attention was drawn to this modification by Lieutenant-Colonel Perry, R.A.M.C., who kindly gave one of us a demonstration of the test and supplied a quantity of antigen.

Summary of Comparison of Sachs-Georgi and Wassermann Test.

	Agree				Disagree		
S_1A	23	1	—	..	5	—	—
S_1P	5	13	—	..	—	—	—
S_2A	24	—	—	..	7	—	—
S_2P	8	3	—	..	3	—	1
S_3A	2	—	—	..	2	—	—
S_3P	1	—	—	..	—	—	—
S latent A	2	—	—	..	—	—	—
,, P	4	—	—	..	1	—	—
S congenital	1	—	—	..	—	—	—
V.S.	—	30	2	..	—	3	—
N.V.	—	7	—	..	—	—	—
No history	39	38	7	..	8	2	4
	109	92	9	..	26	5	5
		210		..		36	= 246
		85·4 per cent.		..		14·6 per cent.	

Note. S_1A. Primary syphilis before treatment.
S_1P. Primary syphilis after treatment.
S_2A. Secondary syphilis before treatment.

Since December, 1921, we have performed by this method 1,148 tests with 1,005 sera. The sera have been taken from batches of sera sent to this laboratory for Wassermann test, the only method of selection being to take sera of which a sufficient quantity remained after the Wassermann test had been completed, no reference being made to the clinical histories.

We have in the main followed the technique described by Dreyer and Ward in the *Lancet* of May 7, 1921, the antigen being prepared according to the formula of Bordet and Ruelens.

We prepared our first antigen before seeing this article and minced the heart as in making an ordinary Wassermann antigen; this antigen gave poor results, presumably as the alcohol had acted as an extractor and not as a coagulator. Since then we have chopped the heart in ¼-inch cubes, and obtained satisfactory antigens.

The test tubes in which the reactions were put up varied considerably in calibre and this caused variations in the readings; it did not perhaps cause a positive serum to appear negative, but it made a difference in the degree of flocculation, and is one of the reasons we have not yet adopted actual numbering of the degree of flocculation according to the Dreyer-Ward calculation.

The following are points of difference in our technique from that of Dreyer and Ward.

(1) The pipette used for dropping the sera was washed in saline only, as in our routine in the Wassermann test.

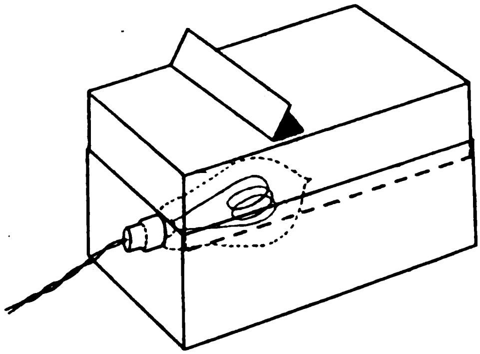
(2) All nine dilutions were put up for each serum as in several cases we found that there was little or no reaction before the fifth tube, while the higher dilutions gave a reaction.

(3) The trays of tubes were taken from the water bath and read at the

end of seven hours, the trays were then placed on the table and a second reading was made at the end of twenty-four hours. It was found that false precipitates in twenty-four hours were rare, and, when present, usually were in sera which were obviously septic ; these precipitates were readily distinguished from true flocculation.

On several occasions flocculation though absent in seven hours was found in twenty-four hours in one or more tubes, and this second reading was considered to be the correct one. Seven-hour flocculation almost always increases to some extent in twenty-four hours.

We are of opinion that a twenty-four hour second reading should always be made.



To help in the reading a device on the lines of that described by Dreyer and Ward was made. It is a cardboard box 9 inches by 5 inches by 3 inches, a slit was cut along the front and a piece of cardboard bent up as a flap over the slit; the front of the box and the inside were covered with black carbon paper ; a hole was cut in the top and an electric bulb put inside the box with the plug protruding through this hole. The tubes to be read were held in front of the slit and flocculation showed up much clearer than in daylight or in front of an open electric bulb. (See sketch.)

It was found that the flocculation showed up better if the tubes were gently rotated between the hand ; however this was never done till the tubes had been first examined.

For a time we attempted to calculate our results according to the Dreyer-Ward method, taking as a standard the extract supplied by Colonel Perry; but we gave this up and give the reading the nomenclature of Dreyer-Ward for each tube, e.g., S. in the fifth tube. The reasons why we have temporarily given up actual calculation of the degree of flocculation are :—

(1) We have not yet obtained standard calibrated test tubes, and Rooke pointed out in the *Lancet* of January 21, 1922, that even with the

tubes sold as standard Dreyer Tubes, variations in size caused variations in reading up to thirty per cent.

(2) We have not had the opportunity of comparing our antigen with a standard Dreyer-Ward antigen.

(3) In comparing different antigens we found that they did not always act in parallel with different sera; this may have been due to differences in the calibre of the tubes used.

The following is a summary of the first 1,148 tests performed by us by the Dreyer-Ward technique.

SUMMARY OF COMPARISON OF WASSERMANN AND DREYER-WARD TEST.

	Agree			Disagree			Agree	Disagree
	Pos.	Neg.	Partial	W + D −	W − D +	W ± D ∓	Per cent	
S_1A (1)	218	17	6	11	8	1	92·3	7·7
S_1P (2)	44	119	11	1	15	3	90	10
S_2A	194	—	—	9	—	—	91·4	8·6
S_2P	97	42	12	4	10	2	92·7	7·3
S_3A	23	—	—	—	—	—	100	
S_3P (3)	6	—	2	1	—	—	88·9	11·1
Latent A (4)	8	—	—	1	—	—	88·9	11·1
Latent P	4	4	2	—	—	—	100	
Non-ven. (5)	8	57	2	1	3	—	93	7
Ven. sore	—	156	14	—	10	3	92·3	7·7
Congenital	1	—	—	—	—	—	100	
No history	11	4	1	1	1	—	88·8	11·2
Total ..	614	399	50	29	47	9		
	1063			85				
	92·6 per cent.			7·4 per cent.				

These tests were made with 1,005 sera, of which 143 were tested with two extracts.

Note:—S_1A Primary syphilis before treatment.
S_1P Primary syphilis after treatment.
S_2A Secondary syphilis before treatment.

(1) S_1A.—Wassermann positive, Dreyer negative. Of these eleven, five were on two consecutive days when all Dreyer tests were negative, most probably owing to the saline used being bad as a leak was discovered in the still. Two of the five cases have been clinically confirmed.

(2) S_2A.—Of the nine disagreeing, three were done with bad saline on the same day as the primary cases referred to above. In addition, in one there was insufficient serum to perform the complete test.

(3) S_3P.—Wassermann positive, Dreyer negative. This case later gave a positive Dreyer reaction.

(4) S Latent A.—Wassermann positive, Dreyer negative. Later gave a positive Dreyer reaction.

(5) Amongst these were included the following presumed to be suffering from non-syphilitic diseases.

	Agree +	Agree —
Leprosy—Nodular ..	1 ..	—
Nervous ..	2 ..	5
Mixed ..	3 ..	1

	Agree —	Agree ±	W. — D. +
Malaria	4 ..	2 ..	1

Psoriasis—Four cases: Wassermann and Dreyer negative.
Rat-bite fever—Agree + 1 (later this patient gave Wassermann negative).

The leper sera and clinical notes were kindly supplied by Dr. Mankad of the Kangra Peth Leper Asylum, Ahmedabad.

We would make the following remarks on the Dreyer-Ward Sigma Test :—

It is very accurate and as reliable as the Wassermann test in the diagnosis of syphilis. We notice several writers claim that this method is more sensitive than the Wassermann test ; some of our tests show an earlier positive by Dreyer-Ward than by the Wassermann test, but it must not be forgotten that the Wassermann test is put up in two quantities as a rule as compared with the nine dilutions put up in the Dreyer-Ward, and one could obtain earlier positives by modifying the Wassermann reaction, e.g., as in Stern's method.

The Dreyer-Ward test compares unfavourably with the Wassermann test in the time taken to put up the test. We can put up 80 to 100 Wassermann tests in one hour, including the dilutions of reagents and testing of the complement, while it takes us about an hour to put up 16 Sigma tests.

The Wassermann test is extremely easily read ; we read eight degrees of hæmolysis in each of the two tubes and find it much simpler to select these figures than to pick out the degrees of flocculation in the Dreyer-Ward test. We find it very fatiguing to read a number of Dreyer-Ward tests, whereas large numbers of Wassermann tests can be read without any fatigue. We admit that in a laboratory where small numbers of tests have to be made, the advantage of the Wassermann reaction does not appear to be so great, but, where large numbers have to be done and the technique is standardized, the Wassermann reaction can be done very rapidly.

The sera tested came from all parts of India and Burma, many having been four to eight days in transit, but this, even in the hot weather, did not seem to affect the reaction.

We have tested very septic sera and badly hæmolysed sera, and have obtained readings agreeing with clinical findings ; such sera would have been useless for the Wassermann test.

The Dreyer-Ward test does not necessitate the keeping of guinea-pigs, rabbits and sheep, and as no amboceptor has to be prepared, a vivisection licence is not required by the worker, who prepares his own reagents.

REPORT ON BACTERIOLOGICAL EXAMINATIONS IN CASES OF INFLUENZA, MALTA COMMAND, 1918.

By ADAM PATRICK, M.D.
Late Temporary Captain, Royal Army Medical Corps.

WORK done in the District Laboratory, Malta, in connexion with the influenza epidemic was chiefly on two lines: (1) bacteriological examination of sputum from patients in hospital, and (2) post-mortem examinations and examination of post-mortem material in the case of some men who died. Nearly all the specimens of sputum sent to the laboratory came from St. Andrew's Hospital, but there were a few also from St. Elmo and Cottonera Hospitals. The sputum examinations were begun about the end of August and continued until the end of December, a period corresponding roughly with the second and severest wave of the epidemic. As large numbers of patients whose sputum was examined were cases of bronchopneumonia which had been transferred to a special block on account of the onset of this complication, they had usually been ill for some time when specimens were sent. An attempt was made to carry out a post-mortem examination on as many cases as possible where death had occurred, but there were difficulties in getting permission in the case of Maltese soldiers. The bodies of nearly all the British soldiers who died were examined.

EXAMINATION OF SPUTUM.

The sputum was collected in wide sterile tubes into which the patient spat directly. The specimens were generally received in the laboratory and cultures made within three or four hours. The medium used for nearly all the cultures was blood-smeared agar made from bullock's heart, though a few cultures in the middle of the series were made on plain agar, when it seemed, after numerous negative results, that the influenza bacillus was not present. A direct smear was also made in each case. These were useful for the recognition of spirochætes, and pneumococci and sarcinæ were also thus easily observed.

Specimens of sputum were examined from 187 patients, and in a few instances more than one examination was made. By far the most common organisms met with were *diplo-streptococci*, probably mostly pneumococci; but pneumococci and streptococci have been classified together as they were not differentiated. These organisms were present in ninety-four per cent of all cases. *Staphylococcus aureus* was found in twenty-seven per cent. In the common infections of the respiratory tract this organism is unusual, and its prominence and virulence were features of the Malta epidemic. *Other Gram-positive cocci*, mostly large, were found in fifty-five per cent, sarcinæ, which were of common occurrence, being included in this group. *Gram-negative cocci* of the *Micrococcus catarrhalis* type occurred in fifty-one per cent, *Gram-negative coliform bacilli* in thirty per cent. These bacilli gave very variable reactions in sugars, but the pneumobacillus was conspicuously absent. *Bacillus influenzæ* was isolated in four

per cent of the cases. The standard taken for its identification was growth on blood-smeared agar, failure of growth on ordinary agar, and greatly increased growth in the proximity of staphylococcus. Spirochætes, much resembling those present in Vincent's angina, were found in twelve per cent of the sputa. Fusiform bacilli were present in large numbers in association with them in one case. *Bacillus septus* was found in three cases.

The following classification shows the organisms found in the sputum of twenty patients who afterwards died, and in the lungs of ten others whose sputum had not been examined before death—thirty cases in all. Here *diplo-streptococci* were present in 76 per cent, *S. aureus* in 83 per cent, *other Gram-positive cocci* in 30 per cent, *Gram-negative diplococci* in 30 per cent, *coliform bacilli* in 33 per cent, *B. influenzæ* in 10 per cent, *spirochætes* in 6 per cent, and *B. septus* in 3 per cent.

TABLE I.

	Organisms in sputum of 187 cases	Organisms in sputum or lungs of 30 fatal cases
Diplo-streptococci	94 per cent	.. 76 per cent
S. aureus	27 ,,	.. 83 ,,
Other Gram-positive cocci ..	55 ,,	.. 30 ,,
Gram-negative cocci	51 ,,	.. 30 ,,
B. influenzæ	4 ,,	.. 10 ,,
Gram-negative bacilli (coliform)	30 ,,	.. 33 ,,
Spirochætes	12 ,,	.. 6 ,,
B. septus	1 ,,	.. 3 ,,

A striking feature in this table is the increased proportion of *S. aureus* in the fatal cases.

The percentage-fatality in connexion with the organisms found is shown in Table II.

TABLE II.

	Number of deaths where organism was present
Diplo-streptococci	10 per cent
S. aureus	31 ,,
Other Gram-positive cocci	8 ,,
Gram-negative cocci	9 ,,
B. influenzæ	37 ,,
Coliform bacilli	12 ,,
Spirochætes	8 ,,

The total number of cases—eight—in which the influenza bacillus was found is too small to give a reliable figure.

Post-mortem Examinations.

The number of autopsies made was twenty-three. Special attention was directed to cultures from the organs, and the results of these cultures are shown in Table III.

The prevalence of *S. aureus* in pure culture in the first half-dozen cases is remarkable, and at first it seemed as though it was the presence or absence of this organism which made the difference between death and recovery. In the first nine fatal cases it was the predominating organism in eight. One of these patients came from Ghain Tuffieba camp, three miles from St. Elmo Hospital, and four from the 1st Northumberland

TABLE III.—Cultures from Organs (Post-mortem).

Case No.	1	2	3	4	5	6	7	8	9	1	11	12	13	14	15	16	17	18	19	20	21	22	23
Heart-blood ..	S	S	S	—	—	—	S	—	S	B, P	..	P	P, S	P	B	S, P	B	—	B	P	—	—	P, S
Right lung ..	S[1]	S	S	S	—	S	S	Str[1]	S	P, S	S, B	S, P	S, P	P	S, P	S, P, B	P	I, Sar	P, S	I, P, S	—	S, P	P
Left lung ..	..	..	..	S	S, P	S	S	Str	S	P, B	S, B	S, P	S, P	P[2]	S[1]	S[2]	B	I	P, S	I, S	B	S, B, P	P, S
Right pleural cavity	..	..	..	..	..	..	P	..	Sar	..	..	..	..	..	..	..	..	..	P	S	S	..	..
Left pleural cavity	..	..	..	..	..	S	P, S, Str	Str[1]	S	B	..	P, B	P, S	..	..	P	..	..	P	..	—	..	..
Spleen ..	S	S	S	S	—	S	S, B	—	—	P	—	B	—	P	B	S, B	—	—	B	—	—	B	—
Liver	..	..	S	S	S	S	S, B	—	—	B	S, B	S, P	P	P	B	S	B	—	—	—	—	B	—
Gall-bladder..	—	S	S	—	—	—	—	—	—	—	—	—	—	—	—	—	B	—	—	—	—	—	—
Right kidney	..	..	S	S	—	S	S	—	—	P, B	..	—	P	P, B	B	—	—	I	B, P	P	—	B	—
Left kidney..	..	..	..	S	—	—	—	—	—	P, B	S	—	—	P	B	S, B	—	I	—	—	—	—	—

(Cultures pure unless otherwise stated.)

[1] Almost pure culture.

[2] Not a pure culture.

S = *Staphylococcus aureus.*
P = Diplo-streptococci, resembling pneumococcus.
Str = Streptococcus.
B = Gram-negative bacillus (coliform).
Sar = Sarcina.
I = *B. influenzæ.*

Fusiliers. These were all the examinations made of fatal cases from these localities. Staphylococci were found in other cases, but not in the same degree of purity as in these eight. There is evidently a great tendency for organisms present in the lungs to become disseminated through the body. Organisms were usually to be found in spleen and liver, and the frequency with which they were isolated from the kidneys is interesting. It will be noted that in Case No. 18 influenza bacilli were grown from the kidneys. It might be mentioned here that cultures made from the spleen in many previous post-mortem examinations in other diseases by the same method as was here used for all the internal organs had given uniformly negative results. The remark already made about *coliform bacilli* found in the sputum applies also to those isolated from internal organs.

As regards the condition of the various organs, the *heart* seemed to be not much affected, the most noticeable change being the dilatation of the right side, and especially of the right auricle. There was no valvular lesion and no obvious muscle changes. The average weight was ten ounces.

The *lungs* were nearly always affected throughout, and in every case some bronchopneumonia was found. In one case (No. 8) in which the organism concerned seemed to be a streptococcus, there was consolidation in both lower lobes, while the other lobes were pink and spongy, but in every other case there was a change in colour and consistence throughout the lungs. Congestion was always present, with the tissue of a deep red colour, and marked œdema was noted in fifteen cases out of twenty-three. This œdema was occasionally extreme, and in one instance the lungs weighed fifty-three ounces and fifty-four ounces respectively. In some cases the lungs showed deep congestion, numerous small hæmorrhages, scattered ill-defined patches of bronchopneumonia, œdema with much frothy œdematous fluid in the bronchioles, and bronchi with deeply congested mucous membrane. In others the consolidation was more massive, the bronchioles were filled with pus (purulent bronchitis), and abscesses were sometimes found in the consolidated areas. The bronchopneumonic process showed a special tendency to progress towards abscess formation, and all intermediate stages were encountered between the presence of abscesses so small as just to be visible and a stage in which the lung resembled a sponge soaked in pus. On section of a lobe the suppurative process was frequently seen in different stages side by side, the areas being usually wedge-shaped with their bases towards the pleural surface.

The extent of the areas of bronchopneumonia varied greatly. Sometimes they were small and scattered, and sometimes they ran into one another and formed a massive consolidation. The lower lobes were more extensively affected than the upper, and the right middle lobe was noticeably less affected than any other part of the lungs. A common distribution was consolidation in the posterior parts of both upper lobes with the anterior parts spongy, the middle lobe free, and the lower lobes completely consolidated except for a fringe along the anterior and lower

edges. The following table shows roughly the distribution of consolidation in twenty-three cases.

TABLE IV.—DISTRIBUTION OF PULMONARY CONSOLIDATION IN TWENTY-THREE CASES.

Lobe	Complete or almost complete	Partial	Absent
Right upper	7	11	5
,, middle	4	2	17
,, lower	17	2	4
Left upper	6	9	8
,, lower	19	2	2

Small hæmorrhages throughout the lung tissue, showing under the pleura as petechiæ, were of frequent occurrence, but extensive subpleural hæmorrhage was found in one case only.

Pleurisy was common, generally in patches, but sometimes involving almost the whole pleural surface. There was effusion of serous fluid in the right pleural cavity in three cases, and in the left in one of these three and in two others. In one case 1,500 cubic centimetres were present in the left pleural cavity, but in the others the amount was small. Double empyema occurred twice, and single left-sided empyema in five other cases. Generally only quite a small amount of pus was found, but one pleural cavity contained about 500 cubic centimetres.

Histological examination of sections of lung tissue showed desquamation and inflammation of the mucous membrane of the bronchi, with dilatation of the blood-vessels and sometimes leucocytic infiltration. Small areas of pneumonia could sometimes be traced to an origin round a bronchiole. Hæmorrhage into the lung tissue and abscess formation were common. In the consolidated areas the alveoli were filled with effusion.

The average weight of the right lung was 33·7 ounces, and of the left 30·3 ounces.

There was no noticeable change in the *spleen*. In particular softening was absent, even in cases in which abscesses were found in the lungs, and staphylococci were grown from the spleen substance. In one such case the organ weighed only 2¾ ounces. The average weight of the spleen was eight ounces, but as several of the patients had previously suffered from malaria, this slight increase in size was probably attributable to the malarial infection.

Œdema was sometimes observed in the *liver*, and in some cases congestion; but in general there was no marked change. The average weight was sixty ounces.

The *kidneys* were congested, and in one instance, where there were abscesses in the lung, the pelvis of the kidney contained a little pus. The average weight of the right kidney was 5·3 ounces, and of the left 5·7 ounces.

All but one of the deaths occurred in the second wave of the epidemic, and this was, I believe, the only fatal case among the military population of the island at this time. The man was taken ill at Ghain Tuffieha camp on July 2, and died in Cottonera Hospital three days later. Though there

were œdema and scattered patches of bronchopneumonia throughout the lungs, the cause of this rapid death was not at first apparent. On culture, however, *S. aureus* was obtained from spleen and heart-blood in pure culture, and in the lungs it was present in large numbers, and was the predominating organism. Apparently the cause of death was staphylococcus septicæmia. This result led to fuller bacteriological examination in subsequent autopsies.

No particular type of post-mortem appearances could be connected with particular organisms. Attention has been directed to the localized character of the consolidation in one case with streptococci predominant. Pneumococcus and staphylococcus seemed equally capable of producing abscesses in the lung. It was sometimes found that though other organisms than pneumococcus were present in the lung substance, the pneumococcus alone was present in the fibrinous exudate on the pleural surface. Pulmonary œdema was found in a marked degree in cases with *S. aureus*, in Case No. 14 where pneumococcus was the predominant organism, and in Case No. 17 where only coliform bacilli were found. The later cases in the series showed much less tendency to abscess formation in the lungs than the earlier. A feature of some of the staphylococcus cases was the occurrence of death with comparatively slight lung changes, and no evidence of disease elsewhere.

A culture was made from the *blood* in five cases of bronchopneumonia, without any growth resulting. One of these was taken two days before death in a case in which staphylococcus was afterwards found in the heart blood. The *leucocytes* were counted in four cases, and were not increased, the numbers per cubic millimetre being 4,000 (staphylococcus case), 7,800, 6,800 (staphylococcus case), and 6,800.

Conclusions.

The results obtained give no clue to the nature of the primary infection, but they show to what an extent this primary infection reduces the resistance of the tissues to the attack of the secondary invaders. The occurrence of *S. aureus* in a large proportion of fatal cases has not, I think, been recorded as a feature of the epidemic in other places. It would be interesting to know if it is connected with the great prevalence in Malta of staphylococcus infections of the skin. It is evident from the numerous published accounts that the secondary organisms found in the lungs vary in different localities, and difference in case death-rates may be due to difference in the secondary invaders. The conflicting evidence as to the presence or absence of the influenza bacillus is possibly to be explained by some such variations in geographical distribution. It is difficult to believe that its apparent infrequency in Malta was due to the unsuitability of the medium employed, for when it grew at all it grew without difficulty. Six of the last specimens of sputum were plated on the K medium recommended by Fildes, Baker and Thomson (*Lancet*, 1918, ii, 697), but with negative results.

Clinical and other Notes.

SHORT NOTES ON THE DEVELOPMENT OF THE SOLIFUGÆ.

By Major J. E. M. BOYD, M.C., F.E.S.
Royal Army Medical Corps.

Although the Solifugæ belong to the Arachnida and not Insecta, therefore do not concern Entomology, it is thought that a few short notes on their development may prove of interest.

Arachnida are grouped in eleven orders, and include amongst others Araneida (spiders), Acarina (ticks and mites), Scorpionidæ (scorpions) and Solifugæ. The latter are spider-like, but differ from all other orders of the class Arachnida in having the segments of the body grouped in three regions, head, thorax and abdomen, like insects.

Alcock gives a very good description of these creatures in his "Entomology for Medical Officers" from which the following description of an adult is taken. "The head carries the Cheliceræ, the Pedipalps, and in the middle of the front border, a pair of eyes. Each chelicera consists of a swollen basal piece and two pincher-like fangs. These are popularly supposed to inflict a most dangerous bite, but all that is actually known about them is that they are used for catching and killing the prey, which consists chiefly of insects.

The pedipalps resemble legs.

Between the bases of the chelicera there is a little horny spout at the tip of which the mouth opens.

The thorax is composed of three independent segments, and carries four pairs of legs which are long, flexible and hairy, and end in the long stalked claws. On the hind legs there is a series of long club-shaped or somewhat racquet-shaped appendages of unknown function, the Malleoli (on the specimens before me as I write there are five of these on each side, ten in all). The abdomen consists of ten distinct segments; the anus opens on the last, and the genital pore on the first. The breathing organs are Tracheæ, which open between the coxæ of the second pair of legs and on the sterna of the second, third, and sometimes the fourth abdominal segments" (Alcock).

The Solifugæ are very common in Iraq where, as in India, they are generally known by Europeans as "Jerrymunglums." They run with almost incredible speed, and when so running hold the pedipalps raised in front of them in such a way as to look like an extra pair of legs, which are not in use. Owing to the speed with which they move they are very difficult to catch. They are nocturnal in their habits, and are often seen in rooms or on walls searching for their prey, which, from what I have been able to observe, consist entirely of insects.

On July 21, 1922, I was sent an exceedingly large and repulsive specimen by Major H. S. Milne, M.C., R.A.M.C., D.A.D.H. Baghdad District. He had caught it in his mess at Karradah. I placed it in an empty cigarette tin, with a wire gauze top, on a little dry earth, and fed it daily on such insects as crickets, small beetles and moths.

Owing to the shape and size of the abdomen, which was nearly round and about the size of a shilling, I concluded that it was a pregnant female, and so decided to watch developments.

On July 23 there was a swarm of crickets in the evening. Many of these, about a dozen, were caught and fed to the captive, all of which were eaten with apparent gusto.

On August 1 a large number of eggs were seen at the bottom of the box, and on counting, these were found to number one hundred and eighty. They were white and opalescent, about the size of the head of a "lace" pin, and exceedingly pretty to look at. Most of the eggs were single, but one mass of five was found. Some of the eggs were placed in spirit.

Later in the day it was noticed that a few of the eggs had split and partly opened, and by next morning all were in the same condition. In one portion could be seen the mouth parts and legs; the other portion seemed to represent the abdomen. Later the outer covering of the egg separated, leaving the young embryos exposed.

In this early stage the anterior portion showed the Chelicera divided into two lateral portions standing out prominently in the front. The legs were slightly curved, and stood out slightly from the body, the pedipalps being turned back over the thorax and abdomen. The abdomen was round, the whole embryo being white in colour.

There was no sign of any movement, and it was frequently thought that the embryos were dead, and the only reason which prevented me from putting them all in spirit was that they did not become hard and brittle, as they would be expected to do if dead. A few were picked out daily and placed in spirit, but little could be made out as regards development with the naked eye, or a hand lens.

On August 11 it was noticed that the embryos were darker in colour. This was thought to be due to the presence of dust, but on looking at them on August 12 it was found that they had developed into a later stage, and that what appeared to be many small but well formed "Jerrymunglums" were present. Some were picked out that were just in the stage of moulting, others had already passed this stage. They were at this time active, but somewhat feeble, and whitish in colour; later the colour deepened somewhat. As far as can be seen by the hand lens they resemble the adults, but no signs of "malleoli" can be made out. This may be due either to their absence in the young stages, or to the weakness of the hand lens. Still, as several have been preserved in spirit, it may be possible to ascertain this point later.

As the natural situation for ovipositing was not known, about half the eggs were kept on dry earth and the remainder on moist. Both batches hatched out at the same time, and appear equally healthy. Some are being kept alive, but the majority have been placed in spirit. On August 20 some of the young are still alive, though never fed. The mother is still alive and has a healthy appetite. She had to be moved to a separate box, as she showed a tendency to eat the eggs soon after they were laid. This may have been due to the fact that, owing to the incidence of "effects of heat" and other duties, I was too busy to keep her fully supplied with food. She now shows no signs of wishing to eat a few of her progeny placed in her box.

It is hoped ere long to capture an adult male in order to find out whether the female will again become fertilized, and also whether the male on completion of his marital duties is devoured by the female, a common occurrence in some orders of this class.

Exhibit at Kelvin Hall, Glasgow, July, 1922.

NOTE ON THE EXHIBIT PROVIDED BY THE ROYAL ARMY MEDICAL CORPS AT KELVIN HALL, GLASGOW.

By Lieutenant-Colonel R. G. H. TATE.
Royal Army Medical Corps.

The exhibit provided by the Royal Army Medical Corps, at Kelvin Hall, Glasgow, during the Conference of the British Medical Association held in July, 1922, dealt with the problems met with on active service, in relation to the provision of sterilized drinking water for troops.

Great interest was shown by visitors to the Exhibition in the models, photographs, and diagrams set out for inspection. The stall was constantly crowded, and it was obvious that a considerable impression was created by the completeness of the details, and the efficiency of the methods which had been worked out for the provision of safe drinking water in the field.

In one or two cases minute inquiries were made on the subject of chlorination of water, and apparently the information was asked for with a view to establishing similar systems for the use of the civilian population in certain districts.

Travel.

NEWFOUNDLAND.

By Lieutenant-Colonel R. HUNTLY NICHOLSON,
Royal Army Medical Corps (R.P.).

When Newfoundland was discovered at the close of the fifteenth century, the corner-stone of the British Empire was laid. It has always remained a part of the British Empire, for while Canada for three centuries was dominated by France, and the Republic of the United States was born after 300 years of British rule, Newfoundland remained as it was.

When John Cabot accompanied by his son, Sebastian, set sail in their two small vessels from the Port of Bristol early in 1497, under letters patent of Henry VII, they sailed west and arrived at Newfoundland and the coast of Labrador. Only five years had elapsed since Christopher Columbus had landed on one of the many islands of the West Indies, yet it was not until August, 1498, that he reached America. These intrepid adventurers from Bristol may be regarded as the actual discoverers of the North American Continent in that they were the first white men to put foot on the mainland itself.

Newfoundland was visited by the Portuguese navigator Gaspar de Cotereal, in 1500, and within two years after that time, regular fisheries were established on its shores by the Portuguese, French and English.

Devonshire men sailed regularly, and even to-day vessels for this purpose sail from Portmadoc.

This year the French have ten steam trawlers, and the Portuguese two on the Banks. It is part of the French naval training to send their boys to the Banks fishing.

It was not, however, till 1583 that Sir Humphry Gilbert landed at St. John's, and formally took possession of the island in the name of Queen Elizabeth. The struggle between Britain and France for the domination of North America was carried on for many years on the Newfoundland coast, and in 1672, after several futile attempts, Newfoundland was taken by the French but was recaptured by the British in the same year, and in 1706 the French made their last unsuccessful attempt on the island.

All territorial rights in Newfoundland that concerned France were recognized by the treaty of Utrecht in 1713. France, however, retained the Islands of St. Pierre and Miquelon, and does so till this day. These islands are a thorn in the side of the prohibition policy of the United States of America, as they are whisky depots. Newfoundland itself is a prohibition country, except that alcohol is recognized as a medicinal agent. The foregoing treaty granted 800 miles of coast line of Newfoundland to the French along with British subjects for handling and drying fish. This gave rise to continual disputes, and the question was finally settled in 1904 by buying out the French rights.

As is well known the island furnished us with a fine regiment in the late war. The antiquarian will find traces of the original aborigines—a branch of the Algonquins, called Borthicks—such as arrow heads, skeletons. stone implements, etc. Newfoundland has a population of 250,000, the population of St. John's being 33,000.

St. John's city in parts resembles Kingston, Jamaica, but the wooden houses are being replaced by stone since the big fire. Of the population of St. John's about 10,000 are Irish. There is an Anglican and a Roman Catholic Cathedral. One can notice descendants of Devonshire stock at Robinson's and in other settlements, Irish in the Codroy, each keeping to their own location.

The harbour of St. John's is still dominated by the old British Fort.

The shooting consists of snipe, duck, ryper and cariboo, the latter of the reindeer family.

Dippers, finches, thrushes, crows, etc., are all represented, but all different from British.

The salmon fishing is excellent: for every fish in our rivers there are at least ten; it is the real *Salmo salar*. The so-called trout, a clear *Salmo fontinalis*, is said to go up to fifteen pounds, I have caught them up to six pounds; they take sea trout and salmon flies. The flies required for salmon are silver doctor, black dose, Jock Scott, etc., as with us size and colour are the essentials. Flies cannot be too small: I have taken fish up to six pounds on small double-hooked trout flies; the fish on the Great Codroy take bigger flies than any other river on which I have fished.

Spoons, minnows, etc., are useful in the early part of the season, but as the water is from snow only small sizes are of any use. Salmon up to fifty pounds are said to have been caught; I have seen casts of forty-one pound fish; my best was twenty-two and a half pounds.

The island is covered with small spruce, etc., and is of volcanic origin; the banks seem to be of the same origin. Tuberculosis is the chief disease, but the Newfoundlander is long-lived owing to the hardy open-air life he leads. The houses are heated by stoves, and are ill-ventilated.

In the north part of Labrador remains of the Esquimaux are to be found, but they suffered badly from influenza in 1915, and from enteric fever said to have been brought from the Chicago Exhibition. Ice surrounds the island in winter. In 1899 I was in ice amongst the icebergs in S.S. *Ottoman*; the field was said to be 200 miles long, by 150 wide, plenty of icebergs being present; this was in the month of June.

I went to the West Coast of Newfoundland via Halifax; it has recovered from its well-known explosion in the late war. It was interesting crossing the steam ferry from Prince Edward's Island to Nova Scotia; the ferry took a large train on American lines, and we crushed through floe ice for two miles.

There is a Moravian Mission in Labrador. The managing director, the Rev. Wilson, told me that it originally started in Bohemia as the "Brethren," but this name became so popular that it was changed to its present one,—why, he does not know; their tenets seem to be on the lines of the Evangelicals in the Church of England. This Mission is found near the Shipki pass in Bashair on the borders of Thibet; they seem a practical people, and teach their followers trades, etc.

The climate is British in the summer. Sandflies, mosquitoes and a small black fly are in myriads. Preventives for the foregoing are an ointment of oil of wintergreen, tar creosote, etc. A solution of carbonate of ammonia relieves the irritation. No malaria is known in the country except that brought from Gallipoli.

Echoes of the Past.

RECOLLECTIONS OF THE ARMY MEDICAL DEPARTMENT, 1857.

By Surgeon Major-General Sir A. FREDERICK BRADSHAW, K.C.B.

I was present at the largely-attended R.A.M.C. Annual Dinner held in London on June 12, 1922, and was fortunate in finding myself at table with very distinguished veterans. The Director-General, Sir John Goodwin, presided and was supported by three predecessors in office, and by further coincidence among those present were three who like myself had served in India as P.M.O. H.M.'s Forces.

I think that I must have been quite the oldest man in the room, and I believe that I may be now very nearly if not really the most ancient of the past members of the A.M.S.

The assembled company of confrères appeared to be a brilliant gathering, so very many wore decorations and medals; and as to myself I felt somewhat discontented with my lack of more inches of stature.

On that occasion my thoughts reverted—as often before and often since—to the very long bygone days when I entered the Army Medical Department, then the official designation; and it has occurred to me that perhaps some recollections of sixty-five years ago might be read with an interest of antiquarian bias.

After "walking the hospital" (St. Bartholomew's) for the three years, the period obligatory in those days, I obtained in 1856-7 the Licence of Apothecaries Hall and the Diploma of the Royal College of Surgeons, London, the two ordinarily taken for constituting the legal qualification for practice. Feeling disinclined to take work as an assistant to a general practitioner, I began to prepare for admission by competitive examination into the Indian Medical Service. But as at about the same time an official announcement appeared throwing open the Army Medical Department to competition I resolved to become a candidate. Previously commissions were obtainable almost mainly through the interest of some personage titled or otherwise influential in the social or political sphere. Although a letter of recommendation was still required, it sufficed if a hospital physician or surgeon of standing vouched for the respectability and good conduct of his nominee. Thus meek dependence upon patronizing favour ceased to be necessary.

I may mention that at the first of the examinations only those were allowed to compete who had served in the Crimea as acting assistant-surgeons. It was at the second, the really first, the unrestricted one of the series, that I presented myself. The ordeal was conducted in May, 1857,

and comprised a written part followed by oral questionings. The examiners were Deputy Inspector-General David Dumbreck, M.D., C.B., Surgeon First Class Henry Pilleau, and Surgeon Second Class Francis Reid, M.D. The candidates considered to have done sufficiently well so far, had next to appear singly before the Director-General, Andrew Smith, M.D., an austere looking chief; he deplored, in my case at least, ignorance of natural history.

Then we were ordered to proceed to Chatham and report ourselves at the great military hospital in Fort Pitt—the Headquarters I suppose of the Department; to be subjected there to a further examination in the practical duties of the profession, and also no doubt for observation of the candidates' personal characteristics.

Fort Pitt Hospital was organized in two divisions, a medical and a surgical. In charge of the former was Surgeon First Class J. S. Prendergast, M.D., and in charge of the latter Surgeon First Class J. C. G. Tice (I believe). Surgeon Second Class George Williamson, M.D., an anatomist of repute, supervised the post-mortem inquiries, the bandagings and other practical details. An old retired officer, Honorary Deputy-Inspector-General William Parry, used to haunt the lecture rooms—I think for recreative occupation. I imagine that he was a bachelor; he died at Chatham a few years later. Work in the wards consisted in writing notes of cases, making diagnoses and performing any small minor operations. One peculiarity of practice attracted my attention; cupping seemed to be rather frequently done, and by order it was always crossed, as a means I understood of identification of deserters or patients in other military hospitals. The quantity of blood abstracted was commonly very small.

The head of the whole hospital was Deputy Inspector-General J. Robert Taylor, M.S., C.B.

Having always preferred medicine to surgery I was very glad to be sent to the medical division and was very fortunate in finding in Dr. Prendergast a kind and considerate superior.

After a stay sufficiently long, as we candidates thought, we began to feel impatient and rather to assail Dr. Taylor with inquiries as to the result of the examination, and as to our prospects of being given commissions. He expressed surprise at our demurring to delay and remarked incidentally that it had not been at all unusual for young men desirous of an army medical career to stay for quite a long time at their own expense on the chance of being accepted; I imagined that those candidates were waiting and hoping for particular regimental vacancies. We explained to Dr. Taylor that it was necessary for us to know quite soon whether it would be requisite for us to see about other employment. I think that our views were made known to the authorities concerned as our names speedily appeared in a *Gazette* with date May 27, the day of the London examination. Antedates were given to those who had served in the Crimea. Perhaps it might be deemed to have some even very small historical interest

were the names recorded here of those who entered the Department in the unrestricted and actual first of the open competition series; it was that first which made an epoch in the history of the A.M.S.

The names were: Nicholas Ffolliott, Yorke H. Johnson, Ebenezer J. Hatchell, Charles J. Kinahan, E. Louis MacSheehy, James Parr, J. Bowyer Baker, Charles G. Lumsden, J. Allen Thornhill, and the writer, A. Frederick Bradshaw. Of all these I alone am surviving, an isolation attendant on advanced age.

In the same May (1857) a great mutiny broke out in the Native Army in India. Troops were hurriedly dispatched (but in sailing ships, steam navigation being still in imperfect stage), there being pressing need for completing to war establishment the regimental medical staffs. The authorities swept in even those candidates who had failed at both the restricted and the opening examinations, and besides antedated the commissions of those who happened to have served in the Crimea.

It was my good fortune to be gazetted to the Rifle Brigade, and I feel convinced that I owed the appointment to the influence of Dr. Prendergast. I know that he asked me if I wished to go to India, and then went to London, where he was well known at the Horse Guards, having been surgeon to Lord Raglan, the Commander-in-Chief in the Crimea.

I cannot refrain from paying a tribute to the memory of Dr. Prendergast. Throughout my army career and until his much lamented death at his home in Bath on November 20, 1869, he was invariably a kind and steadfast friend to me, and very sincerely do I regret that he has passed away.

At the remote period (1857) which I am now recalling to mind, the Department was organized on a regimental system and included an establishment of unattached officers called staff assistant-surgeous and staff-surgeons, many of them preferring a roving life, available as stop-gaps and some waiting for vacancies in Corps.

As to military authority, army doctors had not any at all, but of course men of forceful personality backed by their medical position could exercise influence sometimes by dominating character. Life in a regiment was usually pleasant; comradeship was brotherly and friendships firm, and *esprit de corps* became strongly developed. *Experto crede.*

There was one disadvantage to the Department in that official encouragement did not exist for arousing emulative performance of professional duty. Promotion went as a very general rule by mere seniority; the official theory apparently being held that every medical officer who had not been badly reported upon was as good as any other. Consequently it took a very long time to climb up to the top of the grade or rank; for one instance, I was assistant-surgeon for fourteen years. By the bye, my military medical service had a quaint beginning. To avoid a career in the Indian Medical Service I entered the Queen's; three months afterwards I had to go to India with my Battalion and there I stayed for

twenty-one years before leaving the country, and then was absent from it for only a few months.

One of my earliest military functions in India was attendance at a punishment parade as the junior of the medical staff. The Battalion was drawn up, the delinquent tied up and the operating drummer ready with his whip. The ceremony began with infliction of a vigorous lash which elicited remarks and groans from the recipient; after a few cuts the man emitted a loud moan and sank down apparently collapsed. I went to him and finding the pulse quite normal I said nothing and did nothing but went back to my place. Every eye of the Battalion was then turned towards me, the drummer stared at me in perplexity and being uncertain what to do tried the experiment of a stinging lash on the drooping figure. Instantly the astonished delinquent sprang up like a jack-in-the-box and submitted without further ado to the remainder of his sentence. The case was my first experience with a malingerer, and I suspect that the man was greeted with the comments of his fellow privates. I thought that flogging a soldier was a brutal and degrading form of maintaining discipline and I felt glad indeed when it was abolished.

The difference between the conditions regulating the department sixty-five years ago and those governing the Army Medical Service of the present time is very remarkable, the old Department having been transformed into a military corps and given a free hand in its own sphere; invested with military authority over all grades of patients in military hospitals; in possession now of combatant titles and ranks up to and including Lieutenant-General; with a royal Colonel-in-Chief, Colonels Commandant, and a large force of soldier hospital-attendants and stretcher-bearers; also with a Medical College staffed by military medical professors, a great hospital (Queen Alexandra's), and a fine medical mess. What a change! I remember when the great military hospital at Southampton was commanded by an infantry general officer.

The immense advantage which has accrued to the Army Medical Service since it secured unification and internal independence consists in the high reputation gained by efficiency zealously manifested in recent campaigns and most notably during the great war of giant armies waged in 1914 and onwards. Fortified with the expressed confidence of the nation's warriors, with the vast professional experience acquired in battle conflicts and inspired by earnestly actuating *esprit de corps*, the Army Medical Service of this twentieth century will continue to flourish, deserve well of King and Country, and finely vindicate the motto "In Arduis Fidelis."

In concluding the foregoing references to an ancient phase of organization I think mention should not be omitted of a recent and most noteworthy incident in the long life of the Army Medical Service.

Never before Sir Alfred Keogh became a recipient of it had an Army medical officer been raised to the dignity of Knight Grand Cross of the

Most Honourable Order of the Bath, the premier of the distinctions awarded by the King to soldiers. In addition to this hitherto unique instance of royal recognition of a successful and paramount administrative medical control during the war, His Majesty was graciously pleased to bestow the Grand Cross of the Royal Victorian Order and the decoration of Companion of Honour. Moreover, some civil authorities were not remiss in showing appreciation of duty well done for Crown and State. They invested Sir Alfred with degrees and diplomas, *honoris causa*.

It is very gratifying to the Corps that the eminent services of a member should have been so finely rewarded; and it may be affirmed that Lieutenant-General Sir Alfred Keogh has heightened and widened the prestige of the Royal Army Medical Corps. Long, very long may he live in fullest enjoyment of health, of his honours, and of the admiring respect of all whom he governed and of all whom he otherwise influenced.

Reviews.

FIRST NOTICE.

SURGERY OF THE WAR. Vol. I. Edited by Major-General Sir W. G. Macpherson, K.C.M.G., C.B., LL.D., Major-General Sir A. A. Bowlby, K.C.B., K.C.M.G., K.C.V.O., Major-General Sir Cuthbert Wallace, K.C.M.G., C.B., and Colonel Sir Crisp English, K.C.M.G. Pp. 618, with 16 coloured plates and numerous illustrations. Printed and published by His Majesty's Stationery Office, 1922. Price £1 5s.

The editors are to be congratulated on this valuable account of the progress of the art of military surgery during the Great War, and the publication of the second volume will be eagerly looked for by all those interested in this branch of surgery.

Although much literature has appeared from time to time dealing with the surgery of the war, it has been either in the form of articles in the various journals, or, when in book form, has only dealt with the subject in a limited way; either a particular region of the body being selected or the wounds discussed as they were encountered at some particular region in the line of evacuation from the front line to the home hospital.

Up till now no real attempt has been made to collect all the factors which influence the result of a wound throughout the entire career of the wounded man.

This first volume of the surgery of the war deals with a large variety of subjects all written by distinguished surgeons and physicians who were in many cases directly responsible for the advance made in our knowledge.

The choice of authors has been wisely made, and the only difficulty has been to attribute to each precisely what he is responsible for, as in some instances more than one writer is responsible for an article. The war did much to get the surgeon and physician out of their watertight compartments, and this joint authorship is strongly to be commended, especially when we see a subject like wounds of the chest discussed from both the medical and surgical aspects. The reader who expects to find a text-book on military surgery will be disappointed,

as the volume is designed to deal with the subject historically. As might be expected, the greatest attention is devoted to tried and proven methods which were largely in use at the termination of hostilities.

Although designed as a history, the work is somewhat inconsistent, for some subjects are dealt with in minute detail, while others are only discussed in a broad and general way.

While in many ways it is a convenient arrangement to divide the treatment of the wounded into the various stages from the fighting line to the home hospital, it tends to perpetuate the weak points of all the previously published literature of the surgery of the war. It is probably, however, unavoidable when dealing with the subject historically.

Our last war in South Africa had misled us in many respects, and the unexpected outburst of sepsis, tetanus and gas gangrene found us ill equipped in knowledge, appliances and surgical facilities. The volume rather assumes that the reader is familiar with the conditions that prevailed at the outbreak of war. If a little more space had been devoted to the early months, and the difficulties met with in collecting and treating the wounded at that time more fully described, the claim to be a complete history would have been better established, and the progress made by military surgery would have been further emphasized.

The first two chapters comprising fifty-seven pages of the text and several appendices are devoted to the discussion, by Major-General E. M. Pilcher, of the subjects of international agreements, enemy projectiles, mode of action of projectiles and the results of projectile action. In the early days of the Great War we were all astonished by the variation in type and severity of wounds compared with our last big war in South Africa. Not only did they differ in the degree of trauma to the tissues, but also in the almost universal sepsis that ensued on the wounding.

This masterly article reviews in detail the modern development of the high-velocity sharp-pointed bullet and describes the whole range of modern missiles. The severity of the wounds caused by the sharp-pointed bullet is shown to be the product of high velocity varied by the resistance of the tissue struck, and aggravated by the liability of the missile to turn over on impact. As the actual production of the wound is out of the control of the surgeon, the subject of missiles and their action is too often neglected by the military surgeon, and consequently there is a danger that this section of the book may be hastily passed over in the anxiety of the reader to explore such subjects as the development of the casualty clearing station or the controversy on wound sepsis and antiseptics. No greater mistake could be made, and the reader who omits this section will fail to understand the reasons for the marked difference between the wounds met with in previous wars and the serious and almost invariably septic wounds encountered in the late war. Had we realized earlier that the infection was produced at the moment of wounding and was deeply implanted in the tissues, we might have attained earlier to reliance on surgery to eliminate the sepsis, and the struggle for the perfect antiseptics would never have arisen.

The subject is a very difficult one to discuss in short compass, but the author has succeeded in recording in an interesting and readable manner the development of modern missiles, and has summarized the work of Fessler and Mann in a masterly way.

The statistics given are interesting and, although admittedly meagre, help us in forming an opinion as to the frequency of wounds by the various types of missiles.

The discussion on international agreements only emphasizes the futility of such conventions to control or modify the use of destructive and maiming methods of offence in war. The illustrations both in the text and in the appendices are good with the exception of fig. 4. This is of special interest to the medical services as it gives some idea as to the areas which are likely

to be free from enemy shell fire. It has been spoiled by being so reduced in size that the small print can only be read by an emmetrope with the aid of a magnifying glass.

No single subject, with the exception of wound infection, aroused so much interest among surgeons on the Western front as wound shock, and never before has such an opportunity occurred to study it in all its aspects. Much new light has been thrown on its causation and many old theories have definitely been discarded.

The subject is dealt with from the point of view of the front line area by Lieutenant-Colonel Cowell, and the discussion is continued by Captain J. Fraser, who describes the condition in greater detail, as seen at the casualty clearing station.

The authors have collected and analysed all the theories and factors bearing on the subject, all of which lead to the conclusion that although primary wound shock is a thing beyond the control of the surgeon, the conditions which predispose to the dangerous condition of secondary wound shock are in a large measure within his control, and much may be done to prevent it.

The most interesting part of Lieutenant-Colonel Cowell's article is the account of his study of the blood-pressure of men in the trenches under varying conditions; in the unwounded in quiet spots, under fire, and after being wounded.

His account brings to light one interesting fact. Although much clinical observation of the condition had been carried out by individual surgeons, and many important facts had been realized, such as the benefits of warmth and infusions for prevention and treatment of shock, yet it was not until the summer of 1917 that a special committee of investigation was appointed, and information as to the causes of shock was circulated, and measures to combat it were suggested.

Many lives were saved by the adoption of these methods, and it is to be regretted that the research was not instituted earlier in the war.

The account of how information on the effects of low blood-pressure and stasis of blood in superficial capillary areas was gradually accumulated is given fully, and the charts of blood-pressures are most interesting and instructive.

Captain Parker's hints for the prevention of wound shock are given in detail, and are on the whole excellent, although one or two points are open to criticism from the surgical point of view.

We note that no mention is made of combining an alkali with Bayliss' gum solution for intravenous infusion in shock cases, and although acidosis is probably a result and not a cause of shock, we know from clinical experience that the addition of an alkali is beneficial.

The cases of shock quoted in detail are instructive, and it is interesting to note that shock has been observed in cases where no severe hæmorrhage had occurred, and where there was little damage to muscle. Does this not throw doubt on the theory that the absorption of toxins, the result of muscle injury, is an important factor in the causation of shock? The question of a tourniquet as a means of preventing this absorption requires consideration. An effective tourniquet is very painful and after its removal the restoration of the circulation is again painful, and may actually under both conditions aggravate shock.

The mental condition of the patient and apprehension that the wound is mortal, as a factor in shock production, is hardly alluded to, and although individual variations in this respect are most striking, many cases were seen where this mental factor was the most important one.

Captain Fraser's article is an excellent account and a clear analysis of the accumulated knowledge on the subject, and should be closely studied by every surgeon and every administrative medical officer.

His description of the pathology of the condition makes clear the correct methods of prevention and treatment.

While there is general agreement on the ill effects of low blood-pressure and concentration of blood in superficial capillary areas leading to loss of blood in effective circulation, there is still room for further investigation as to the exact cause or causes of wound shock.

No one of the factors mentioned appears to account fully for the condition, some are the effect and not the cause, but even without this precise information, it is evident from the exhaustive account given that our knowledge of this vicious circle and the methods to combat it have been greatly added to by the researches carried out during the war.

The enormous number of wounded, many suffering from the effects due to hœmorrhage and shock, gave a great impetus to the methods for transfusion of blood.

At first reliance was placed on infusions of various kinds, and although these were much improved especially by the introduction of Bayliss' gum solution, it soon became evident that however useful these might be to raise temporarily the blood-pressure, the only efficient oxygen carrier was blood.

The knowledge gained is reflected in the greatly increased number of transfusions of blood now practised in civil life.

The subject is fully discussed, and the various methods are described in a convincing manner by Colonel Gordon-Taylor. Not only is the subject dealt with historically, but the article goes into such minute detail that the chapter would be well suited for a textbook.

The citrate method is advocated by the author who points out that in an emergency nothing but a needle, a bowl, and some citrate solution is necessary for drawing the blood.

This method had frequently been used with complete success although it is preferable to utilize a Robertson's bottle if it is available.

The remarks on transfusion of blood in cases of septicæmia and chronic wound infection are worthy of note, and bear out the general experience of the war, although in recent months more progress has been made in this direction.

The statistics given of mortality in serious abdominal cases where transfusion was used are instructive, and may lead us to alter our view as to when an abdominal case is moribund.

As the testing of the serum of the recipient against the cells of the donor is a much more useful method than that of placing both in their proper blood groups, it would have been an improvement if this simple procedure had been given in detail.

One of the surgical surprises of the war was the occurrence of the serious affection known as gas gangrene in the wounded.

Few of the surgeons at the Front had ever previously seen a case, and we have a lively recollection of our first real experience of this condition after the crossing of the Aisne.

Much of the knowledge we now have of the disease and the correct surgical methods of dealing with it we owe to the efforts of Sir Cuthbert Wallace, and he contributes a clearly written and well illustrated article on the subject, which sums up our present knowledge of gas gangrene and details the stages by which this knowledge was obtained.

The bacteriology of the subject is not dealt with in detail in this part of the book. This is rather to be regretted as without it the article does not give a complete account of the condition, and a precise knowledge of the types, conditions and method of growth of the various anaerobic organisms involved in the disease is a necessity for the surgeon who is going to treat gas gangrene. The inclusion of a coloured drawing showing the microscopic changes in muscle would also have been an improvement.

A short account of the trial of a prophylactic serum is given. This is illustrated by statistics which show that the results were disappointing, but the reasons for this failure are not fully discussed.

If the armistice had not occurred when it did an opportunity would have occurred of using the triple serum, already employed by the French, and the results would probably have been improved.

No account is given of the work of the special Committee on Gas Gangrene.

The broad outlines of surgical treatment are well described, but the inexperienced surgeon would require more detailed information on the subject of muscle resection as opposed to amputation.

Some muscles lend themselves readily to excision throughout their entire length, whereas in other cases removal of a muscle or group of muscles may be a far more serious operation than amputation.

The fact that gas gangrene occurred in some cases of trench foot is not mentioned.

Chapter VII, consisting of sixteen pages with one whole page diagram, is devoted to the subject of tetanus, and in it Sir F. W. Andrewes recounts the history of this serious affection during the War, and the steps taken to diminish the incidence, and to improve the treatment of the established disease.

The statistics given, which have all been previously published by Bruce and Cummins, are convincing proof of the value of serum prophylaxis, but still leave us in doubt about the value of serum treatment when once the disease has occurred.

The most interesting part of the article is the account of seventy cases seen personally by the author in his capacity as Tetanus Inspector in one of the London areas. Attention is specially directed to his account of cases of recurring tetanus.

All the points made by the Tetanus Committee are brought out in the article, and some credit is given to early efficient surgery as a factor in diminishing the incidence of the disease. That this was not an unimportant means for reducing tetanus is borne out by the influence early surgery had in the prevention of gas gangrene.

During quiet periods on the fighting front when every wounded man could have his wounds operated on at the casualty clearing station, gas gangrene at base hospitals was much reduced, and it is certain that the incidence of tetanus, also due to infection by an anaerobic organism, was at the same time diminished.

It is interesting to note that two important points are affirmed by the author, although both have recently been questioned. The first is that the administration of a general anæsthetic lessens the danger of anaphylactic shock, and the second is the recognition by Tullcch that the human intestine may harbour the tetanus bacillus.

An article on tetanus is always incomplete without the bacteriology and experimental work on the subject, including the question of anaphylaxis, and although the experiments of Sherrington are alluded to, the important work of MacConkey on the question of dosage in treatment is not mentioned.

The addition of some of the diagrams produced by Bruce and Cummins would have added to the interest.

This is the only place in the volume where any reference is made to the total number of wounded in the war.

In the short space occupied the article covers the ground in an able and interesting manner and is an excellent account of the additions made to our knowledge of the disease by our experience during the late war.

The subject of trench foot is very briefly dealt with by Colonel Grattan.

As far as the article goes it is good, but it is too condensed to do justice to such an important cause of wastage as this condition proved to be.

The figures given are useful, but would have been more valuable if expressed in terms of the total troops exposed to the risk of trench foot.

This article is robbed of much of its interest by deferring the discussion of the preventative measures to one of the volumes on the "Hygiene of the War."

Chapter IX, which is devoted to the discussion of anæsthetics during the War, is not a very convincing article.

As a general statement regarding the anæsthetics found most suitable for seriously wounded men it is quite sound, but are there no records of series of cases to support these statements and show their relative values?

A great deal more might have been said about the value of intraspinal glucose stovaine solution especially when combined with light ether anæsthesia. At one base surgeons were convinced that this was life-saving in serious amputations of the lower extremity.

The most interesting record in the chapter is that relating to the employment of lady nurses as anæsthetists, and suggests that this should be part of their training in times of peace.

The author points out that septic cases are bad subjects for anæsthesia, but does not draw attention to the harmful effects of repeated anæsthetics for the purpose of performing painful dressings, which has such a marked effect in lowering the resistance of the patient to infecting organisms.

Lieutenant-Colonel Max Page contributes a readable and fairly complete account of surgery in a field ambulance as it was seen in the later periods of the war, when a fixed line was the rule.

The author recognizes this, and carefully points out that these conditions do not always obtain and he draws attention to the surgical work necessarily done in field ambulances in the early months of the war before casualty clearing stations had begun to function.

The early treatment of hæmorrhage and the methods to prevent the onset of secondary wound shock are emphasized, and attention is drawn to the futility of preliminary sterilization of the wound until complete surgery can be applied.

The removal of wet clothes as advocated was seldom accomplished on account of the lack of anything to replace them.

Allusion is made to the question of infusions and to the possibility of transfusion of blood by the citrate method.

The danger of the tourniquet is emphasized and the first duty of the field ambulance officer is to remove it, and very seldom was bleeding seen to follow such removal.

We are glad to see the completion of the field medical card alluded to as one of the most important duties of a field ambulance officer. The card might have been described in detail and the date of its introduction given.

The subject of splints and first aid splinting of fractures is soundly but shortly summed up. We would have liked to see the swivel ring Thomas' arm splint given a place, as it so effectively got over the transport difficulty and rarely caused injurious pressure on the axillary vessels or nerves as the amount of extension applied need never be great.

The very convenient skewer method for securing extension and obviating rotation in fracture of the femur is omitted, and no mention made of the special extension spat which was issued.

The question of utilizing Field Ambulances as advanced abdominal centres is rightly condemned.

We entirely agree with the author's contention that sterilized dressings in small packets for Field Ambulances should be adopted. This would bring us into line with the other great nations and be a real economy. No mention is made of the compressed antiseptic gauzes and wools issued in the original equipment or how ill adapted they were for the work.

The article is a really good one, and should help officers in future to realize the limitations of surgery in a field ambulance.

If some idea had been given of the surgical equipment in a field ambulance at the start of the campaign, and the additions made to it during the war, the interest would have been increased.

No part of the book will be read with greater pleasure and profit than Sir A. Bowlby's article on the "Casualty Clearing Station."

As Advising Consulting Surgeon to the forward areas he had unrivalled opportunities of seeing the surgical work of the casualty clearing stations, and this chapter gives a most interesting account of how this medical unit which at the outbreak of war was only meant to act as a collecting and distributing zone for casualties, developed into the most important surgical centre in the field, where more was done to save life and limb than in any other area.

The chapter opens with a description of the original unit, including its personnel and equipment, and then are described its developments through the several great battles of the war, until eventually we see the almost perfect machine of 1918.

The author fully recognizes that this was only possible in conditions of practically stationary warfare, and shows how in the earliest periods and later when active movement commenced, they could not function so fully as operating centres.

The figures, now given for the first time, are most instructive, and deal with the number of wounded in our various great battles, the proportion of certain types of wounded, and the number of cases actually submitted to operation at the casualty clearing stations.

The methods of diffusing the knowledge of the surgical lessons learned from one casualty clearing station to another and from front to base are described.

The value of team work and the importance of skilled nursing at casualty clearing stations are well brought out.

The description of wound treatment is short and devoid of detail, and is simply directed to showing how the various antiseptic methods gradually gave place to the ideal surgery for the wounded, namely, complete excision of the wound and primary, delayed primary or early secondary suture. Only a very few of the striking points have been mentioned, for the whole article is full of information and must be read to be fully appreciated. This article alone makes the book indispensable to every Army Medical Officer.

(*To be continued.*)

MATERIA MEDICA AND THERAPEUTICS. By Bruce and Dilling. London: Cassell and Company, Ltd., 1921. Pp. xiii. + 678. Price 10s. 6d.

The twelfth edition of a well known standard work needs little more than announcement of its publication. Familiar to students and practitioners for many years, the present edition upholds the reputation of its predecessors, and at the same time has been the subject of careful revision.

Subjects noted for the first time are the Rôle of Vitamins in Deficiency Diseases, Detoxicated Vaccines, Benzyl Benzoate, Ethanesal, and Delayed Chloroform Poisoning, while two entirely new sections have been introduced—one on Natural Mineral Waters and Baths, the other on Invalid Diet, which, in the opinion of the authors, receives too little attention from the student of medicine.

J. A. A.

SURGICAL DISEASES OF CHILDREN: A Handbook for Students and Practitioners. By Frederick C. Pybus, M.S., F.R.C.S. London: H. K. Lewis and Co., Ltd., 1922. With 228 illustrations; pp. 408.

The author in his preface gives a fair indication of the scope of the book when he states "The volume does not pretend to be a complete treatise, but

rather a record of personal experience of the commoner conditions met with on the surgical side of a children's hospital."

Students presenting themselves for their final examinations who read this book in conjunction with a more extensive one on general surgery, will have a good knowledge of the surgical affections of children.

While some of the articles are excellent, other surgical conditions are so briefly described that they might with advantage have been omitted. Some, indeed, do not go much further than a definition of condition. As might be expected tubercular and syphilitic affections occupy a considerable place, and this is one of the best parts of the book.

The author inclines to optimism in the prognosis of tubercular joint and bone disease in children. The description of the technique of operations is very brief but the methods of treatment are eminently practical.

One of the great features of the book is its wealth of illustrations, which are in most cases excellent. The skiagrams, which are well selected, are on the whole good although some have evidently suffered in the process of reproduction.

There is a very complete index and the volume, which is free from typographical errors, is excellently printed.

It should be of value to students and practitioners for whom it is designed.

THE SURGICAL TREATMENT OF NON-MALIGNANT AFFECTIONS OF THE STOMACH. By Charles Greene Cumston, M.D., and Georges Patry, M.D., Lecturers at the University of Geneva and Members of the Surgical Society of Switzerland. With an introduction by Sir Berkeley G. A. Moynihan, K.C.M.G., C.B., M.S. London: William Heinemann.

For a review of a scientific book to be useful to the readers of the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS it should contain certain definite information.

It should indicate the class of officer to whom the book will be valuable, and give some account of the subject matter of the book, with any special points of appreciation or criticism.

With regard to the first of these headings, the book is definitely written for the practising surgeon.

The physician will find little to guide him in the treatment of non-malignant affections of the stomach with the exception of the important advice, often reiterated, that medical treatment should not be too long continued, if it fails to effect a cure, before handing the case over to the surgeon.

The surgeon will be disappointed if he refers to the work for the description of how a particular operation is performed. The authors have designedly omitted all such descriptions, although valuable hints on technique are scattered through the book.

It is hardly a book that would be read with profit by a medical student, as it presupposes knowledge which he is unlikely to possess.

For the subject matter of a book, reviewers have been known to rely on the author's preface, but in this case we would quite fail to do justice to the work. The authors' claim is that it groups together the data acquired during the past twenty years in the domain of gastric surgery. A perusal of the book shows that this claim is well founded, but it fortunately goes much further and gives us the views of the authors.

The authors state that few statistics are given. We found a good many, and the book would have been less valuable had they been omitted.

Sir Berkeley Moynihan in his introduction states that "neither the physician nor the surgeon undertakes adventures into the territory of the other with that frequency which would be a benefit to both." This book strongly emphazises the fact that the treatment of a gastric or duodenal ulcer is only begun when the

excision of the ulcer, gastro-enterostomy or other operation is performed, and that many of our surgical disappointments can be attributed to the failure to follow up the operation by appropriate medical treatment over a long period.

The opening chapter deals with the history of the various operative procedures and gives a fair and unbiased account. It is burdened with a good many dates and mentions many continental writers with whom we are scarcely familiar in England. Under the heading of gastro-enterostomy, which is ably discussed and strongly advocated for many conditions, one important detail is described which we have not seen before. The advice is, to make the opening in the stomach slightly longer than that in the intestine. The authors argue that as the intestine is thinner and less retractile than the stomach, there will be more material on the jejunal side, and when suturing is completed, folds will have been produced which may be the starting point of valve formation. This extra tissue on the jejunal side must be a common experience with all operators, and adds considerably to the time spent in suturing if a safe and neat approximation is to be secured.

The description of unsuccessful cases is extremely valuable, and is an example which might with advantage be followed by other authors.

It is interesting, at a time when the actual length of the loop in a posterior gastro-enterostomy is being much discussed, to find the authors say "we do not know a case in which a short loop has been supposed to have been the cause of vicious circle or bad function of the stoma."

The authors consider that the Y-shaped anastomosis should be discarded and produce good evidence in support of this view.

We do not agree with the authors when they state "post operative adhesions have not been the cause of serious vicious circle." It is, however, comforting to the surgeon to be told that the formation of adhesions is independent of the operative technique to some extent.

Much has lately been written about peptic ulcer. The authors tell us not to be too pessimistic about it. It is interesting to note that Murphy's button still holds a place in the technique described, but the case for it is reasonably put forward.

Under the heading of resections a strong plea is made for a preliminary gastro-enterostomy followed after an interval of ten days by the resection.

The use of the cautery in excising ulcers is warmly advocated.

In dealing with stenosis the discussion is good, and for certain cases where the parts are pliant the advantages of Finney's operation are discussed.

We can hardly agree with the statement that "exclusion of the pylorus is little known and rarely resorted to." Temporary exclusion, at any rate, has been extensively practised in this country.

The mortality of operation for congential stenosis is put down at 3 per cent. In our experience the average is much higher than this.

Under the heading, Pathogenesis of Gastric Ulcer, the authors give one of the best discussions we have read on the subject, and although the conclusions reached leave the matters still open, it is a concise aud able argument.

The surgeon likes to have at his command a book from which he can get help in a difficult case. We have put this book to the test on a severe case of gastric hæmorrhage and it was not found wanting.

It may seem ungraceful to offer any criticism of a book which is so full of valuable information, and written by men who are clearly masters of the subject. The style is somewhat laboured and the book must be read at a time when the mental faculties are alert, it is not a work to trifle with over a comfortable fire after dinner.

There is a great deal of unnecessary repetition which is probably due to the methods of discussion adopted.

Some of the terms are unpleasing to an English reader, as for instance "return to the norm" and "gastric·chemism."

The book, which has no illustrations, is clearly printed and well produced by the publishers.

A TEXTBOOK OF THE PRACTICE OF MEDICINE. By various authors. Edited by F. W. Price, M.D., F.R.S.Edin. Oxford Medical Publications. Pp. 1642. Price 35s.

This publication covers the ground ordinarily required by students and practitioners of medicine. To secure uniformity of method and style, the Editor has generally adopted the principle of allotting a whole section to one author or to two writing in collaboration. The result is a very excellent production, which includes useful Sections on Immunity and Immune Therapy, Skin Diseases, Tropical Diseases, and Psychological Medicine. The selection of twenty-five authors, each eminent in his particular sphere, ensures an up-to-date treatment of the subject matter, which will be very welcome to the practising physician anxious to keep abreast of modern methods and ideas.

The section on Diseases of the Circulatory System is particularly good, and is fully illustrated by reproductions of pulse tracings, electro-cardiograms, etc., so essential to a clear understanding of circulatory disturbances. From the point of view of the practising physician the chapter on "Cerebrospinal Fever" is the best exposition of the subject that one has seen in any general textbook. It is particularly pleasing to see the vital necessity for the early administration of curative serum so emphatically stressed, and the commonest reason for delay in commencing such specific treatment is also emphasized—confusion of early cerebrospinal fever with influenza. The allocation of space to some of the diseases is difficult to understand. It seems strange that chlorosis should be dealt with in a page and a half, whereas nine pages are given to the consideration of trench fever.

In several instances the zoological nomenclature is not up to the standard which should obtain in such a book. The discarded synonym *Filaria sanguinis hominis* appears unqualified on p. 1072, and in the section on Diseases of the Skin the nomenclature of the human lice is very inexact. An author's name is cited in the manner directed for sub-specific terms, and although head and body lice are stated definitely to be merely racial varieties of "*Pediculus humanus linnæus*" (sic), their names appear in the guise of valid species. Also we are told, apparently as a means of diagnosing, that the head louse has "a powerful mandible." It is a pity that so excellent a publication should contain such confused statements; doubtless they will receive attention from the Editor in a future edition.

The value of the book is much enhanced by the care which has been given generally to treatment, and there seems no doubt of the fulfilment of the Editor's hope that the textbook will be considered a credit to the London School of Medicine.

THE INTENSIVE TREATMENT OF SYPHILIS AND LOCOMOTOR ATAXIA BY AACHEN METHODS. By Reginald Hayes, M.R.C.S. London: Baillière, Tindall and Cox, 1922. Pp. vii and 99.

The fourth revised edition of this work on the Aachen system of treatment of tabes and syphilis is an index of the interest and success of this form of treatment in intractable neuro-syphilis. The photographs, in the text, of the position taken by patient and rubber indicate the value of strong pressure in inunction. Each photograph shows the operator in the best mechanical attitude to press the mercurial ointment into the skin. Most syphilographers would not agree with

the secondary place given to the arsenical preparations, but all who have had experience in the treatment of tabes acknowledge the high value of mercury in the form of expert inunction as advocated in this monograph. A. T. F.

A TEXT-BOOK ON GONORRHŒA AND ITS COMPLICATIONS. By Dr. Georges Luys. Translated and edited by Arthur Foerster, M.R.C.S. Fourth revised edition. London: Baillière, Tindall and Cox, 1922. Pp. xi and 400.

This standard book on gonorrhœa still holds the field as the highest authority on the disease. Luys upholds the view that an expert can cure the chronic disease by patient obliteration of diseased urethral foci. The success of catherization of the ejaculatory ducts is further emphasized in this edition, and gives hope of a higher percentage of cure of this potent cause of gonorrhœal relapse.

Luy's book can be recommended as containing all the latest ideas on the subject.

The indexing of the book still leaves room for much improvement, it is hoped that this point will receive attention in future editions. A. T. F.

Notices.

EDITORIAL NOTICES.

The Editor will be glad to receive original communications upon professional subjects, travel, and personal experiences, etc. He will also be glad to receive items of news and information regarding matters of interest to the Corps from the various garrisons, districts, and commands at home and abroad.

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Original Communications.

THE INFLUENCE OF MILITARY SERVICE ON HYGIENE.[1]

BY MAJOR-GENERAL SIR W. G. MACPHERSON, K.C.M.G., C.B., LL.D.

THIS time last year Sir Percy Bassett-Smith addressed you, as your President, on the "Progress of Hygiene, Pathology, and Bacteriology in the Royal Navy." The subject on which I propose to address you this evening is "The Influence of Military Service on Hygiene," although from a somewhat different point of view, and more in the form of historical retrospect than as an indication of progress. But before doing so I should like to rectify a grave omission on my part—and I may say also on yours—in not referring at our last meeting to Sir Percy Bassett-Smith's work as President of the group during the past year. Sir Percy entered on his duties imbued with that enthusiasm for which he became so well known not only in the Royal Navy but also in many circles outside it; and during his year of office he has won for himself our admiration and affection. I hope, therefore, that you will join me in expressing to him our deep appreciation of the services which he has rendered to the group.

When he handed over the chair at our last meeting, Sir Percy referred to the work of Major Daukes, as Secretary of the group, and I would ask you to express our cordial thanks to him also for his services. His task was by no means a light one and I am sure we all regret that his work is making such demands on his time as to oblige him to resign the secretaryship. At the same time we give a hearty welcome to Flight-Lieutenant W. H. Wood for so kindly taking up the burden which Major Daukes has thus been compelled to throw off.

[1] Presidential Address delivered before the Navy, Army and Air Force Group of the Society of Medical Officers of Health, December 1, 1922.

In approaching the subject of "The Influence of Military Service on Hygiene," I should explain that my object is to show historically the direction in which armies and the demands of armies dealt with health problems rather than to present to you the manner in which hygiene as a whole benefited by military service. From the historical notes, however, which I have ventured to put together, some indication may be gathered of the extent of the influence which military service brought to bear in connexion with questions of health preservation and prevention of disease.

The maintenance of physical fitness, physical training, the hygiene of the march, the relationship between food and energy, camp sanitation and all that it implies are aspects of hygiene which belong specially to the domain of military service. But there are other features (such as the prevention of epidemics and preservation of health in communities of individuals living in close contact with one another) which have been influenced to a great extent by the experience of armies in the field and military service in general, although they have an equally important bearing on civil life. The subject of medical topography also owes much to the experience of military service in various climates and countries. If the influence of military service, therefore, were to be explored fully, a subject of unwieldy dimensions would present itself; and I propose to touch on a few only of its more salient historical features this evening.

The maintenance of physical fitness and individual health is the earliest manifestation of the hygienic sense, if I may so call it. It assumes a prominent position in the history of campaigns from the earliest times; for it implies the man-power which enabled a nation to defeat its enemy and win battles. Consequently we find much in the writings of the ancients and in military literature at all times regarding the selection of recruits, and the training, feeding, clothing and housing of the soldier. In fact from the principles underlying these and the measures taken to prevent armies being attacked by epidemics, the science and practice of hygiene may be said to have emerged and to have expanded into civil life.

It was the army, for example, which constructed the public works, sewers and watercourses in ancient Rome, drained swamps and engaged in other sanitary work. As Lieutenant-Colonel Garrison of the United States army puts it, "the general sanitation of the Roman Empire was largely the work of the army." In this connexion it is interesting to note that the United States appears to have followed the example of the Romans, for the United States corps of engineers is employed in peacetime in the construction of public works. No better instance of this need be cited than the construction of the Panama Canal, which became possible only after the work was undertaken by the engineers and medical services of the United States army.

The selection of the best type of recruit is a subject which crops up constantly both in ancient and modern writings. Vegetius, for example, who wrote on the art of war in the fourth century A.D., devotes a whole

chapter to it, discussing questions of physique, age and previous occupation. He lays great stress on the graduated training of men, recruited from cities and accustomed to luxurious and sedentary lives, and compares them unfavourably with the hardier and stronger men from rural districts. In modern times the importance of this subject is indicated by the physical classification of the manhood of the nation during the recent war, and the voluminous and meticulous data published recently by the United States military authorities. Even Moses appears to have found it necessary to estimate physical fitness, by numbering the men "from twenty years old and upwards, *all that were able to go forth to war*." Constant reference will be found, therefore, to the selection of the best type of men physically and mentally from the earliest times up to that most recent development of the subject in the experimental work which was carried out in the military physical test station in Edinburgh by Professor Henry Briggs in 1918.

With regard to physical training the exercises and sports of the ancient Greeks, in the open air and stripped of clothing—gymnastics or gymnics in other words—differ in principle but very slightly from the doctrines preached in more recent times. Alexander the Great issued a strict order to his soldiers to take care of their bodies, and gymnastics and sports were their chief amusements. Vegetius, whom I have already referred to and will refer to again—as of all the ancient military writers he appears to have been the greatest exponent of military hygiene—states that those skilled in the art of war considered that daily exercises did more than the doctors to preserve the health of the soldiers. Amongst more modern hygienists, Sir John Pringle, the Physician to the Forces in the War of the Austrian Succession of 1741 to 1747, expressed the opinion that more sickness was caused by inactivity than by fatigue, and recommended encouragement of sports of all kinds, with a caution as to excess. A mean between love of ease and pursuing the most violent exercise was the principle advocated by him.

Robert Jackson, than whom there has been no greater or more courageous and adventuresome army medical officer, although he died in obscurity in 1827 in his 77th year, unnoticed by the authorities of his day, had much to say on the subject of recruiting, training and exercise, repeating practically the observations of Vegetius. A whole section of his remarkable work, "A systematic view of the formation, discipline and economy of armies," published in 1804, with a second edition in 1824, is devoted to these subjects. He extols dancing, fencing and gymnastics as important elements in physical training; and he enforced the principle of graduated training and of carrying out exercises in the open air. Jackson's attempt to influence hygiene is expressed in the words of his biographer, Dr. Borland: "He had a pervading anxiety respecting the health and well-being of the soldier."

It is curious to note how soon the principles enunciated by Jackson

and the earliest writers were forgotten or ignored; and it is to this fact that we must attribute the recommendations which appear, in strong protest, from time to time in the writings of subsequent military hygienists, so much so that, as recently as 1908, a committee had to be appointed by the War Office to investigate the physiological effects of food, training and clothing of the soldier, with the result that, so far as training was concerned, all that could be said was that it should be graduated and carried on out of doors, as far as possible. The ancient Greeks and Romans told us that; the great military hygienists of the three last centuries told us it; yet in 1908 we had to produce a new system of training, in accordance with these principles, in the "Manual of Physical Training" published in that year.

The opposition of military commanders to the advice of medical officers in this respect was no doubt due to the influence of the Prussian system of drill introduced by the father of Frederick the Great and to the writings of Clausewitz, whose classical work "On War," written in 1827, and published after his death from cholera in 1831, has been the Bible of the general staff officer, at any rate of the German staff officer. The theory that tall men, with stiff necks and "ramrods down their backs," were the acme of military efficiency seems to have dominated the military hierarchy for more than a century after the time of Frederick the Great, in defiance of the wisdom of men like Pringle and Jackson and indeed of the great military commanders of ancient times. In an old pamphlet of 1799, for reference to which I am indebted to Mr. Hudleston, the War Office librarian, the following significant passage occurs: "Such an army as he (General Burgoyne in North America) had was not fit to fight in woods, composed of heavy useless Germans and high dressed British infantry." The Austrian troops of the period are alluded to in the same pamphlet as "high dressed, stiff and unwieldy."

Clausewitz's influence appears to be still more pernicious, for he states that the theory of sanitary measures has not such an influence on strategic decisions as to make it worth while to include a consideration of them in the theory of war. The first English translation of Clausewitz's book "On War" appeared when Lord Wolseley was commencing his great career, and I wonder whether it could have inspired his well-known opposition to the sanitary officer. "The sanitary officer," he says in his "Soldier's Pocket Book" published in 1886, "is the creation of recent years, and as a general rule he is a very useless functionary." In any case I feel sure that had Lord Wolseley lived to witness the spread of sanitary organization into every nook and cranny of the field, and the marvellous activity of the sanitary officer and his influence in maintaining man-power and efficiency during the recent war, he would have expressed differently his appreciation of the value of sanitation.

But this is digressing somewhat from the subject of physical training; a subject which carries us on to the hygiene of the march. Here again we find remarkable evidence of how well the military commanders and

writers in early times understood the importance of the principles preached by modern military hygienists.

It is difficult to realize the extraordinary marches recorded in ancient times. They make one doubt the veracity of historians. Herodotus, the Father of History, was also called the "Father of Lies," and, if I remember rightly, our modern Pepys, Colonel Repington, quotes M. Briand as saying that "history is a lie promoted to the rank of truth by repetition." However that may be, we have in Xenophon's narrative of the march of Cyrus' army of Persians and Greek mercenaries from the coast of Cilicia against Artexerxes in the year 401 B.C., and the subsequent retreat of the Greeks from the battle of Cunaxa south of Baghdad, up the Tigris and across the mountains of Kurdistan and Armenia to Trebizond on the Black Sea, a record of a march of 3,465 miles in 215 marches averaging over 15 miles each. This record was surpassed some seventy years later by Alexander the Great's famous expedition, as told by Arrian, into India through Persia and Afghanistan and back from the mouth of the Indus through Baluchistan.

Another famous march, quoted by historians, is the march of Marlborough's army from the Meuse to the Danube before he fought the battle of Blenheim. Here again some historian has made an error as to the distance and time, for Lieutenant-Colonel Garrison in one of his articles this year in the *Military Surgeon*, quotes it as a march of 1,176 miles in 86 days; whereas the distance as the crow flies is not more than 300 miles, and two military books which I have consulted make the march either 250 or 300 miles, although the time of the shorter distance is given as six weeks and of the longer just over a month.

But whatever may be the truth regarding these historical marches, all are agreed that they were only possible by strict attention to the hygiene of the march and the food, clothing, equipment and individual care of the soldier.

The pace of Cyrus' march was apparently three miles per hour, and this has been regarded as the most economical pace by all military hygienists. It was proved scientifically by experiments made by Cathcart and others during 1917 and 1918. They showed that the expenditure of energy in marching at a pace much slower than 90 yards per minute (that is to say a little over 3 miles an hour) was much greater than marching at that rate or indeed at a rate up to 130 yards per minute.

Almost the only contribution of Clausewitz to military hygiene is in connexion with the march; for he points out that a succession of even moderate marches is certain to tell on the instrument and that a succession of severe ones will, of course, do so much sooner. He regarded six or nine English miles as the most economical distance for an army to travel daily and was strongly in favour of periodic rests. But in discussing the subject of rests he makes some observations with which no hygienist will agree. Apparently the objection in his time to prolonged rests was that soldiers

would then be confined to crowded camps and billets; but he considered that this could be ignored because, to quote his words, "it is so easy to give them while at rest in packed quarters both air and exercise so that the want of these can never be a cause of sickness"!

In more recent times the hygiene of the march has been closely studied in its physiological aspects. A remarkable and standard work was published by Marey on "Le Mouvement" in 1868. Thurn in 1872 wrote on the diseases caused by severe marches, and in 1901 Zuntz and Schumburg published a volume on the "Physiology of the March" in V. Coler's well-known library of military medical books. In 1905, a French writer, Manœuvrier, wrote a short monograph on methods of classifying recruits based on estimating scientifically their capacity for marching.

Investigations of a similar character have been carried out by military medical authorities in England, but more in connexion with the expenditure of energy in relation to food. Thus in 1875 Edmund Parkes carried out an experimental march from Netley to test the restorative values of rum, coffee and meat extract; and in 1909 an experimental march, known as the "hunger march," was made by twenty-five soldiers under the medical charge of Lieutenant-Colonel Melville, Major Beveridge, as he then was, and Captain Dunbar Walker of the R.A.M.C. to investigate the amount of food required by men on active service and the composition of the iron ration. A second experimental march was made under the same direction in the following year.

During these marches many points were noted, such as the importance of marching with open jacket and shirt and of graduated and progressive training. The results were embodied in the Infantry Training Manual of 1911.

The chief consideration, however, in connexion with marches and especially prolonged marches, such as those historical ones which I have just mentioned, was sufficiency of food. All historians agree that the success of the march of the Persians and Greeks in Xenophon's time and of the British to Blenheim in Marlborough's time was due to the careful arrangements made for feeding the soldiers, as well as to the regulation of pace, daily length of the march, periods of rest and individual care. It is this relationship of food to work, and the calculation of the requirements and constituents of a soldier's ration, that form perhaps one of the most important features in the influence of military service on hygiene. Until more modern times, of course, scientific calculations of the value and nature of food constituents were not made; but there is much that is of interest in the general organization of the ancients for feeding armies during prolonged expeditions.

Let us take, for example, the arrangements made by Cyrus in his march to Babylon. They have close resemblance to those of modern armies, and consisted of

(1) A regimental train, organized for carrying sufficient food for each unit to last from one source of supply to another.

(2) A three days' halt for rest in a locality where there was abundance of supplies and where areas for requisitioning supplies could be exploited.

(3) Trading canteens, forming a mobile market, where troops could supplement their rations by purchase.

(4) A reserve supply column for providing rations when the distances from one supply area to another were too great for the supplies to be carried in the regimental train.

The essential constituents of the ration in Xenophon's time were cereals, fresh meat, fruit, vegetables, cheese, wine and oils; and there is no record of those deficiency diseases which were so prevalent in the campaigns of the Romans, in the Middle Ages and in more modern times.

Both Pringle and Jackson devoted considerable attention to the question of the soldiers' diet, but they discuss its nature and amount more from the point of view of what is good for the soldier to take than from the point of view of physiological constituents.

Jackson sums this up by saying that the diet should be "wholesome in kind and spare in measure." He believed in Spartan diets and Spartan habits. Pringle also laid much stress on the evil effects of excess in food, as well as on the dangers of such diseases as scurvy arising from scarcity of food. He has many pertinent remarks on the subject. He believed scurvy and malignant fevers to be due to putrefaction of the blood, and that this could be prevented or retarded by the use of foods that had, in his opinion, antiseptic qualities. In fact he carried out a series of experiments for the purpose of noting and standardizing the efficacy of various substances in preventing or delaying putrefaction. An account of these experiments was submitted to the Royal Society in a series of seven papers read before it between June, 1750 and February, 1752. Pringle's belief in the value of certain constituents of diet in counteracting putrefaction of the blood is expressed in the following quotation:—

"Hopped beer, wine and vinous liquors, coming more and more into general use, have been some means of suppressing putrid diseases. Greens and fruits are likewise more universally eaten, and salted meats make a much less part of our diet than formerly. To this add the more general consumption of tea and sugar, which I have shown elsewhere to be no inconsiderable antiseptics."

In more recent years military problems led to detailed scientific investigations on the physiological importance of the chemical constituents of food and their relationship to expenditure of energy. Much of this investigation was initiated by Pettenkofer and Voit in Germany, but also by the Rev. Professor Haughton in this country. In military service it attained much prominence in the writings of Edmund Parkes and his successors; more especially when concentrated and preserved foods became more or less essential in the Army in order to reduce bulk in transport and at the same time give the soldier a ration sufficient to maintain his physical fitness and vigour. The estimate of the chemical constituents of every

variety of food and their conversion in terms of calories into work represent the direction in which military service influenced this branch of hygiene. During the recent war and when the shortage of food was felt, much additional knowledge was obtained in this respect by the work carried out under the Food (War) Committee of the Royal Society in 1917. New tables of food values, supplementing the well-known American tables of Atwater and Bryant, were then prepared by Plimmer, and these, combined with the work of Cathcart and Orr on energy expenditure, have a scientific value which is bound to exercise a lasting influence on the hygiene of food in relation to work.

Associated with this we have the exhaustive investigations into food deficiency diseases and their prevention, which resulted from the experiences of the Great War, notably those carried out in the Lister Institute, by Miss Hume and Miss Chick and other able lady workers, together with the practical experience of the physicians in Egypt and Mesopotamia; although we must credit the Japanese for going far long ago in the direction of preventing and eliminating the cause of food deficiency diseases in their army and navy.

Another important result of the war was the careful supervision of the manufacture of preserved foods. I remember in the South African War, too, when complaints reached the War Office of the calf's foot jelly supplied to hospitals, I was deputed to inspect the factories, where some interesting facts throwing light on the cause of the jelly going bad were noted. Again only last week Professor Kenwood told us in his lecture at University College how the war had increased the practice of milk pasteurization in the milk trade.

While on the subject of food it may be of interest to refer to the many controversies that have taken place on the value of the so-called "rum ration." Wines and vinous liquids have always been a constituent of the soldiers' ration in the armies of ancient Greece and Rome, and in the present day in the armies of wine-growing countries. Rum is a peculiarly British ration, dating probably from the time we owned sugar-growing colonies. Consequently we hear little about its effects before the time of Robert Jackson, who spent much of his service in the West Indies, and who was strongly opposed to its issue. Pringle, however, some fifty years earlier was a great advocate of the value of wine and alcoholic drinks as a constituent of the ration, as I have already indicated. He was probably influenced by his experience on service in Flanders and Scotland.

"As to spirits," he writes, "it is to be observed that even when drunk in excess they tend more to weaken the constitution than to produce any of the common camp diseases." And again, "soldiers have often to struggle with the extremes of heat and cold, with moist and bad air, long marches, wet clothes and scanty provisions. Now to enable them to undergo these hardships it is proper that they should drink something stronger than water or even than small beer, which is commonly new

and bad in camps." He considered that the danger was greater from excess in eating than in drinking, and quotes Celsus on this point: "Si qua intemperantia subest, tutior est in potione quam in esca." Pringle, it may be mentioned, drawing a conclusion from his theory of blood putrefaction and the antiseptic qualities of wine, referred to the greater prevalence of pestilential fevers amongst the Moslems than amongst neighbouring Christians as being due to their abstinence from wine and fermented liquors. Parkes' experimental march in 1875 appears to have settled the question and to have established the present military view with regard to the "rum ration," namely that it is of value at the end of a day of fatigue and exposure, or in order to induce special effort over a very short period of time. This is exemplified by the evidence given before the recent War Office Committee on shell shock by Lieutenant-Colonel Rogers, who had a very special knowledge of soldiers in the trenches as Regimental Medical Officer of the 4th Battalion of the Black Watch. "Had it not been for the rum ration," he said, "I do not think we should have won the war. Before the men went over the top they had a good meal and a double ration of rum and coffee." The ration, as you know, is only issued on the recommendation of the Medical Officer.

But on controversial matters such as these, to use the words of Jackson when he discussed the hygiene of clothing, "opinions of men fluctuate like the tides of the ocean."

We come thus to the influence of military service on the hygiene of clothing. In 1901 a War Office committee dealt with the question and introduced the present field service dress, on principles of hygiene which had been preached long before. It was the Prussian influence to which reference has already been made that kept us back, and against which Jackson strongly protested. He makes scathing remarks on the "spit and polish" of the army. "Pipe clay," he says, "is employed to cover dirt." "A soldier notwithstanding he might be encrusted from head to foot was said to be clean if his small clothes and facings were covered with pipe clay, and his head was said to be dressed if the hair was matted with a pasty of grease and flour." Against this Jackson emphasized the importance of clothing which gives freedom of movement to the chest and limbs and of boots the natural shape of the foot, such as did the War Office Committee of 1901. Similar views were held by the Maréchal de Saxe in 1738, when he issued orders for greasing hands and feet as a protection against cold and for the abolition of foot and leg clothing which impeded the circulation. Nor were these principles of hygiene ignored by Xenophon, who issued orders for the prevention of trench foot or frost bite which were practically the same as those issued in France during the war; for his narrative records that the lacing of the shoes of his men caused constriction with resulting frost bite and gangrene, and under a rigid system of discipline he ordered his Captains to see that the men removed their foot gear at night, rubbed and dried their feet, anointed them with

oils and fatty substances, and were kept moving or employed in exercises such as felling trees to maintain bodily warmth.

We come now to camp sanitation, a subject more than any other on which military service has taught us most. When the children of Israel fled from Egypt "with their armies" and prepared for warfare in the Land of Promise, Moses, as all who read the Bible know, issued laws which were to be obeyed in war, and drafted regulations for camp sanitation not unlike those of the present day, for they include such basic principles as the isolation of infectious diseases, the prevention of venereal disease, care in the selection of food, sanitary disposal of excreta and waste products, and sanitary inspections. The priests were the sanitary officers authorized to see that the regulations were obeyed. How far similar sanitary measures were carried out by future generations in Palestine cannot be traced, but Lieutenant-Colonel Garrison notes that the Saracens in the Middle Ages had a system of sanitation which included the digging of trenches for dead animals and excreta outside the camp and filling them in with earth, the selection of pure water supplies and the detection of impure sources.

In the military writings of the Greeks and Romans camp sanitation takes a prominent place. Vegetius more especially devotes space to this subject, saying that measures for preserving the health of troops comprise the selection of camping grounds, water supplies, shelter, and so on, and that if an army is left too long in the same area during summer and autumn, the fouling of the ground, the pollution of the water supplies, and aerial infection spread epidemics which may cause its destruction. His remedy was frequent change of camping grounds and sanitary inspections. In his day there were camp commandants, who seemed to have command over the medical services, and to them and to commandants of labour companies was entrusted the duties of sanitary officers. These duties were, in many respects, similar to those of the sanitary officers in the recent war, and included sanitary inspections of tents, huts, and constructions generally.

Pringle held strongly to the opinion that pestilential fevers were caused by foul sites of camps and insanitary billets. His theory was that the remittent and intermittent fevers of the army in Flanders originated in putrid air, and that the infection spread from fæces and foul latrines and from the occupation of low-lying marshes and damp ground, and dirty and overcrowded billets.

Jackson never went so fully into questions of camp sanitation as his predecessor, Pringle, but urged the importance of medical topography. Medical topography, in his opinion, was as important as military topography, and disregard of it was destruction of the forces in the field. This was certainly so in the case of the ill-conceived Walcheren expedition of 1809 and of many other expeditions, such as that of the French in Madagascar in 1895 and indeed to some extent our own occupation of the Struma

Valley in 1916. Napoleon well knew the importance of medical topography, for he realized what would happen to the British in Walcheren when he refused to send a force against them, saying that within three months fever would finish them, and in Napoleon's correspondence of August 16, 1811, there is the following passage: "Il vaut mieux la bataille la plus sanglante que de mettre ses troupes dans un lieu malsain."

In connexion with camp sanitation, we have the problem of water supplies and their purification. Purity of water was not ignored in the military services of ancient times. Although we do not hear very much about the methods of purification, there are a few indications that military commanders were not indifferent to its importance.

I am again indebted to Mr. Hudleston, the War Office Librarian, for the following quotation from Herodotus regarding Cyrus the Great in his warlike expeditions: "There is also carried with him water of the river Choaspes, which flows near Susa, for the King drinks of no other; where-ever he goes he is attended by a number of four-wheeled carriages, drawn by mules, in which the water of Choaspes, *being first boiled*, is disposed in vessels of silver."[1]

Sterilization of water by boiling was, therefore, no new discovery; but it appears to have been a luxury, granted only to the mightiest in the land, although possibly it may have been more generally practised than history records.

Again Arrian tells us that Alexander the Great took care that those who came to the water first should not run into it with their feet and thereby render it unwholesome for the rest of the army.

The Greeks and Romans added vinegar to water to render it palatable and wholesome. Jackson refers to this and himself advocates its use when water is flat or mawkish, but was opposed as usual to the British adding rum for the same purpose. He does not go further, however, in connexion with water supplies, than to mention the use of alum for clarifying water.

Vegetius considered muddy and polluted water a danger to health; and required all officers to pay particular attention to the question of water supplies and the diseases caused by bad water, from the first day of a campaign. He was referring specially to dysentery.

But it is only in modern times that the hygiene of water supplies was investigated scientifically; and much of the progress in this direction has been due to military service. Chemical impurities were first investigated and limits of permissibility of chemical constituents standardized. The "Manual of Military Hygiene" by Edmund Parkes went a long way to disseminate knowledge of this kind. Later, as we all know, bacteriology

[1] I had been under the impression that the passage which I had read a long time ago, had occurred in Xenophon or Arrian, and that it referred to Alexander the Great, and it was only after searching their anabases carefully and failing to find it that Mr. Hudleston came to my rescue.

took the field and changed the character and standards of water analysis. At our last meeting Colonel Anderson told us of the wonderful work which was done in the war to purify water supplies. In this direction military service has brought us to a stage in hygiene in which the most foul and poisoned water can in an incredibly short time be clarified, sterilized and converted into a table water.

Reviewing the whole question of water supplies and camp sanitation, one is impressed by the fact that advances in consequence of modern civilization have only been technical in character, and that the principles underlying this branch of hygiene were realized in the military service of the ancients as much as in that of the present day. The means for maintaining cleanliness in camps, disposing of waste products and purifying water can only be described as technical advances in hygiene; and in these technical advances the British Army has undoubtedly taken the lead. The history of sanitation in the recent war forms a compendium of information on the subject.

Passing now from the influence of military service on camp sanitation, or what might be called sanitation by improvized measures as distinct from municipal sanitation, we come to the influence of military experience on the hygiene of habitations. Pringle and Jackson were apostles of this branch of hygiene. "The want of pure and wholesome air," writes the former, "cannot be compensated by diet and medicine." "The best rule is to admit so few patients into each ward"—speaking of hospitals—"that anyone unacquainted with the danger of bad air might imagine there was room to take in double or triple the number." "I have generally found those rooms most healthful where by broken windows and other wants of repair the air could not be excluded." Pringle also recognized the value of open fires and chimneys for ventilation and describes a form of ventilator. He recommended the dispersal of the sick into a number of small hospitals, instead of concentrating them together in one large overcrowded hospital, and had thus in his mind the germ of a pavilion system of hospital construction.

Jackson urged disinfection of walls and floors in barrack rooms; and specially the prevention of overcrowding in transports, a subject which military hygienists have been constantly impressing upon the authorities in recent times. He noticed the ravages of contagious fevers in transport ships, describing them as terrible; and considered it important that in taking up a transport, the character of the ship, its wholesomeness and freedom from infection should be investigated, that there should be a definite tonnage allowance per man, that the height between decks should not be less than seven feet, and that there should be proper equipment of bedding and arrangements for sleeping, accommodation for sick and convalescents, general means of ablution, etc., and careful inspection and classification of troops before embarkation.

There is probably no branch of hygiene in which the teachings and

experience of Pringle and Jackson suffered so much neglect as the hygiene of dwellings, of hospitals and of transports. It was this neglect that led to the onslaught on the military and medical services by Florence Nightingale in the Crimean War, with all the subsequent results. Modern hygiene, in fact, as set forth in Parkes' immortal classic, "A Manual of Practical Hygiene," dates from that war. The Royal Commission appointed in 1857 to inquire into the Army introduced reforms which gave the army medical officers authority to advise commanding officers on all matters affecting the health of the troops whether as regards garrisons, stations, camps and barracks, diet, drill, duties or exercises. (Previously medical officers were only entrusted officially with the care of the sick.) Parkes wrote his manual to meet these demands, but found that, as the general principles of hygiene were involved and could be illustrated by examples drawn from army life, it might be made applicable to civil as well as to military communities. It is fortunate that Edmund Parkes was appointed to the Chair of Hygiene, the first chair of its kind, in the new Army Medical School, for no one of higher attainments or wider philosophical outlook in connexion with hygiene existed then or subsequently. As Florence Nightingale says of him in one of her letters, "his knowledge and instruction, diffused from the Army Medical School as a centre, has extended and will extend wherever the English language is spoken and beyond."

The Crimean War had thus far-reaching results in the application of hygienic principles to practical sanitation both in civil and military life. It led, amongst other details, to the introduction of standards of cubic and superficial space in houses, barracks, hospitals and institutions; to the pavilion system of hospital construction; to standardizing of hospital statistics, such as those introduced by Dr. Graham Balfour in the Army Medical Department Reports, and to many other inquiries into problems of the preservation of health, prevention of disease, and the incidence of epidemics. The Barracks and Hospitals Improvement Commission which followed the Royal Commission on the health of the Army entered into details of warming and ventilation, drainage and water supplies, all of which had a bearing on civil as well as on military constructions. It was in this direction that military service at that time had its greatest influence on hygiene, and in fact led to the Public Health Acts which followed the Crimean War.

The story of the influence of military service on the prevention of specific diseases is a long one and there is time only for a superficial glance at it. Generally speaking this influence only commenced to be felt within the last half century. Up till then the knowledge of specific causes of disease had not been determined with scientific accuracy, and it was not until bacteriology, protozoology and entomology assumed the importance they now have that definite progress was made. Previously measures of quarantine were relied on for the prevention of epidemics, if

we except the vaccination methods of Jenner in the prevention of smallpox. Quarantine methods were in vogue from early times. According to Garrison, quoting Sudhoff, the wars of the Middle Ages brought extensive epidemics of bubonic plague and led to a system which included "sanitary control of incoming vessels, observation stations, isolation hospitals and methods of disinfection, all of which measures have been adopted in modern times, in more definite and rigorous form, but with relatively few changes." These measures were adopted first by the great trading ports of Venice and Marseilles in the fourteenth century.

It was military service in Malta that brought to light the causes of Mediterranean fever. David Bruce's discovery of its micro-organism was made while he was a young army medical officer there in 1886, and the demonstration of the micrococcus in goats' milk by Horrocks, in Malta, in 1905, led to the total elimination of the disease from military life. Service in Malta added also to our knowledge of sandfly fever, through the investigations of Marett and Birt, following those of Doerr in Austria.

Prophylactic inoculation against enteric fever was first tried in the South African War by Almroth Wright. Leishman perfected the vaccines and methods of inoculation in the laboratories of the Royal Army Medical College with results in the recent war that form one of the most brilliant chapters in the history of disease prevention.

The American war with Spain had an almost equally great influence, for it led to the determination of the mosquito transmission of yellow fever and to the practical elimination subsequently of that disease from localities where it was previously endemic. It was this war, too, that drew attention to the influence of flies in transmitting disease. The observations made in the camps in Florida proved that flies carried fæcal matter to food and from that time onwards the disposal of excreta by incineration and the prevention of fly-breeding have been routine measures of sanitation. It is interesting in this connexion to note that Pringle very nearly arrived at the truth with regard to the influence of flies in transmitting such fæcal diseases as dysentery. In discussing the causes of dysentery he says: "And here is an old observation that such seasons as produce most flies, caterpillars and other insects, whose increase depends so much on heat and moisture and consequently in corruption, have likewise been most productive of dysentery." "The first source of infection seems to be the privies, after they have received the dysenteric excrements of those who first fell ill." He even went further and refers to a curious dissertation by Linnaeus regarding the observations of a Danish doctor on the presence in dysenteric stools of minute mobile "animalculæ" scarcely visible to the naked eye.

With regard to malaria, military service has taught nothing beyond the practical measures necessary to deal with the appalling and destructive outbreaks in armies which are forced to carry on operations in notoriously malarial areas, or which in peace occupy garrisons in malarial countries.

The discovery of its causation was due more to independent investigators, such as Manson, Ronald Ross and Laveran, than to those in military service, but an enormous amount was accomplished during the recent war in determining the habits of the mosquito carriers of the disease, more especially in Macedonia and Palestine, and in the malarial topography of these countries.

So, too, the Great War has had a profound influence with regard to the prevention of typhus fever, relapsing fever, and trench fever, and their association with infestation by lice and other insects.

The Great War is responsible again also in connexion with cerebro-spinal fever and influenza for establishing the greater importance of lateral space than cubic space in determining conditions of overcrowding, and for applying this principle to the slinging of hammocks in transports.

Then again we must attribute to the influence of the war the investigations into the factors which produce and prevent food deficiency diseases, as I have already touched upon; the methods of transmission of schistosomiasis, the extraordinary propagandism and general activity in efforts to prevent venereal disease, and many other important practical details of sanitation. The work of combating venereal diseases during and after the Great War has torn the veil, which concealed them from the public eye, ruthlessly asunder. Incidentally Colonel Reece has drawn my attention to a curious line in Horace's Epistle to Quintus, which shows that diseases in his day, too, were concealed by a pernicious sense of shame: "Stultorum incurata pudor malus ulcera celat."

All of these are so much of the nature of recent knowledge, and so well known to the members of this group of the Society of Medical Officers of Health that I need do no more than merely indicate them. Future generations will be better able to estimate their influence on hygiene than we are in a position to do now. But I might venture the opinion that, had there been no such incident as the national military service of the last war, there would have been no Ministry of Health in this country to-day.

But what seems to me an important result of military service is the education of the individual in sanitary habits. I have always held that sanitation depends more on the individual than on the system. A perfect system is useless if the individual does not apply it. Now the individual experience gained in military service is a great factor in aiding the sanitation of civil communities, especially in the less advanced villages and towns. In speaking to me some years ago Surgeon-General Stechow, of the German army,, emphasized this, and said that the training of the soldier in sanitary habits had a wide influence in civil life when he completed his service with the colours and returned to his home. Again, we have the enrolling of the priests in the sanitary sections of the Italian army during the recent war, in the hope that they would spread a knowledge of hygiene amongst their parishioners after they were demobilized.

Individual sanitary education, too, formed an important feature in the

Japanese army during the Manchurian Campaign, and was introduced into the British Army as a consequence of the lessons learnt in that war. One cannot, therefore, attach too much importance to the sanitary education of the individual, and no better means of teaching individual sanitation exists than military service in the field.

In conclusion, I am deeply sensible of the many imperfections in the matter and manner in which I have presented to you this evening a subject that is full of fascination to those interested in the history of hygiene. I am fully aware of the fact that very many points have been omitted and overlooked in connexion with our knowledge of hygiene and its practice under the influence of military service. Still more am I conscious of the fact that the influence of the workers in civil life on military hygiene has been far greater, especially in modern times, than the influence of military workers on civil hygiene. The sanitary work during the Great War bears eloquent witness to this, and the nation can never repay what it owes to the medical officers of health and specialists in hygiene who were enrolled from civil life in the Royal Army Medical Corps during the war, and by whom the bulk of the practical work in maintaining a high standard of health and efficiency was carried out.

I fear I have wearied you with a jumble of suggestions that will only leave in your minds a sense of there being "no intrinsic newness others have not had before." I have merely ventured this evening to put before you a few notes from such literature and impressions as have come my way during forty years of army employment, during which the problems of hygiene and sanitation have occupied no inconsiderable portion of my time; and I have to thank you for listening to me with so much patience and indulgence.

EARTHQUAKES.

By Colonel Sir ROBERT FIRTH, K.B.E., C.B.

My daily paper[1] tells me that yesterday houses were shaken and windows broken in the Plymouth district, owing to earth tremors; and only last week the same paper reported the loss of over a thousand lives and extensive destruction of buildings in Coquimbo, Chile, as the result of an earthquake. Having had an extensive experience of earthquakes, both in Peru and India, the subject interests me, and, since it may interest others, I am tempted to write this article.

Primitive man attributes ground shakings and volcanic activities to the movements of subterranean deities or monsters. Although we do not think so ourselves, still many people have very hazy ideas as to the true nature of these recurring phenomena. That earthquakes and volcanoes occur frequently together in the same country has led to the notion that the latter are the cause of the former. Such is not the case, though the former may be the cause of a sudden activity in the latter. Residents in India not infrequently experience earthquakes, but they reside many thousands of miles from any volcano. History reveals many incidents where cities have been destroyed or buried by the eruptions from volcanoes, but few records exist of volcanic earth tremblings having shaken a town to pieces. Careful records show that the world experiences some 60,000 shakings every year. The great majority of these are mere tremors, unappreciated by most people and registered only by sensitive instruments specially designed for the purpose; their duration may be anything from a few seconds to a minute or more. Because the position of the origin of these tremors can be traced frequently to or along geological lines of fault, the modern view assumes that earth tremors or earthquakes are the result of slight adjustments or settlements of the earth's superficial crust along those geological lines of fault. Ordinary earth tremors are known technically as *microseisms*, and they may disturb an area of greater length than breadth. Out of the world's annual crop of earth tremors, about 150 shake the whole of our sphere, or at least disturb a hemisphere. These are what are commonly called earthquakes, but known technically as *megaseisms*. In the wake of these and originating in the shattered area which they have caused come, during the next few months, the microseisms already mentioned. We are not concerned, in this article, so much with these children or followers of the megaseisms as we are with the large originating earthquakes or megaseisms themselves.

Few countries are entirely free from one or other of these two classes of earth-disturbance, but both are more frequent in places where the rocks are geologically young, and where we have high mountain ridges with

[1] November 18, 1922.

correspondingly deep valleys or sub-oceanic troughs. In other words, where surface corrugations are most pronounced both mega- and microseisms are frequent. It is largely a question of steepness of slope. The Pacific ocean area is a notorious centre for earthquakes, and there the slope from mountain tops to the bottom of the adjacent ocean is as steep as one in twenty. On the Atlantic side, the corresponding slopes do not average more than one in 230, and earthquakes are rare. Japan is rich in young-growing folds of rock, and their adjacent troughs with steep slopes yield some 1,200 earthquakes yearly. In Great Britain, where the rocks are geologically old, we find evidence from huge faults of past megaseisms, but now, since all has settled down, we get annually in these islands perhaps only some half-a-dozen of very small earth tremors. Instruments of various types, designed to record earth tremors and shakings, exist in various countries, and their records afford interesting information as to the character of seismic motion.

A very big earthquake can disturb our earth from pole to pole and throughout its mass. The first phase of disturbing motion or *m*.1. passes through the earth at a velocity of some twelve miles a second. Following on its heels occurs a second phase of motion or *m*.2. which travels at a velocity of only some six or seven miles a second. These phases or waves are probably distortional in character. In addition to *m*.1. and *m*.2. there are other waves which, known as *m*.3., travel parallel to the surface of the earth at a velocity of about four miles a second. These tertiary waves appear to be of a *quasi*-elastic gravitational character, and cause the crust of our earth to rise and fall like a raft or boat on an ocean swell. Doubtless many small earth tremors reported from time to time are due to these tertiary waves. The inference is permissible that the *m*.3. waves are not propagated through the heterogeneous materials which constitute the mass of our earth, like the *m*.1. and *m*.2. are propagated, but that they really are transmitted through some uniform medium beneath the earth's crust. It is from the duration of *m*.1. or *m*.2. waves, or by the time interval by which any one phase outraces any other phase, as recorded at any given station, that the distance of the origin from which the megaseism came is calculated. The movements known as *m*.3., although they may not be felt at any great distance from their origin, may accelerate or retard the swing of a pendulum, alter the rate of a clock, interfere with the accuracy of a balance, and even produce disturbances in magneto meters. These latter disturbances are not felt at all observatories. This fact suggests that these special effects are not produced mechanically. Probably they result in a disturbance of magnetic material in the vicinity of the observatory where they are noted. Since magnetic rock is denser than that which is not magnetic, we should expect observatories near volcanic rocks to respond to crypto-seismic movements more than others remote from these rocks. This is found to be the case.

A megaseism may last a few minutes or some hours. The reason of

this is that the length of time it causes the ground to vibrate at any particular spot depends upon the intensity of the initial impulse and the distance from its origin. As it travels its head and tail disappear first, whilst its body, represented by large waves, radiates at first in ever-widening rings and then, after they pass the equator of their origin, converge. In that manner a large or intense earthquake in New Zealand may create there and in Australia very serious effects and disturbance; its effects may be unrecognizable or unrecorded in India, Egypt and Central Europe, but yet be just recordable in Great Britain, which is near the antipodes of its origin. From certain observations of this kind it appears certain that some megaseisms may burst into life, apparently die out, and then burst into life again. A prevalent idea is that an earthquake increases in its duration as it radiates away from its origin. This is incorrect, but one disturbance on reaching an unstable region may bring into being a second earthquake which may be greater than its parent. For this reason many seismograms do not refer to a single earthquake but to a series which, originating some minutes after each other, may yield records covering three or four hours. Many instances are known of these secondary or multiple disturbances, and one small earthquake may, therefore, create even larger or more severe ones than itself, and these in turn become the parents of further settlements or dislocations. That the large surface waves of a megaseism should cause regions in a state of strain to give way suddenly is not remarkable, and the general result of a large earthquake may often be that earth-strain throughout the whole of our globe has been relieved. The direction in which earth-tremors travel presents some interesting features. It has been noticed that small earthquakes are disinclined to pass at right angles to or travel under mountain ranges, and that the direction of their greatest efforts or activity is usually parallel to the axis of mountain ridges. It is difficult to say why this is so, but it may be that the mountain mass or its foundations or roots, or perhaps both, may form a barrier to the waves of motion. Another point worth noting is that megaseisms which do not shake the whole sphere seem to travel farthest in some particular direction. Thus most of those which originate north of the earth's equator travel right round the northern hemisphere, but do not reach an equivalent distance southward. It is not easy to explain this. Certain observations connected with lunar disturbances of gravity indicate our globe to have a greater rigidity in an east and west direction than it has in the direction of the meridian. If this be so then wave motions arising from an earthquake naturally will be transmitted further in a direction parallel to the equator than at right angles to it. If we create waves in a fluid by a horizontal impulse they travel farthest in the direction to which the primary impulse was exerted. If we apply this fact to microseismic radiations, we must assume that the primary impulses at the various origins were exerted mainly in an east or west direction. Another view suggested is that it is easier to propagate

motion parallel to the Asio-European ridges than at right angles to the same; whilst another hypothesis is the assumption that parallel to or inclined at a small angle to the equator there exists in our globe's mass a system of huge dykes not easily disturbed by the surface movements of the viscous layer in which they are assumed to have originated. From these considerations it is obvious that we have much to learn yet.

Coming to the question of the causes of earthquakes, we find various theories have been adduced to explain their origin. A megaseism evidently arises from the sudden relief of a strain, or a series of strains, in or beneath the earth's crust. The difficulty is, how to account for these strains. Changes in level due to accumulation of water or tidal loads on the crust may reasonably lead to stress or strain. Even after a megaseism, which causes half our globe to palpitate for half an hour or a period of hours, the strains must be enormous. Any strain, however produced, may be tangential or radial, compressional or tensile, and be developed gradually or suddenly. One cause for adjustment or relief of strain may be that the figure of the earth would always tend to adjust itself to its rotatory speed. By a decrease in the rapidity of rotation the stresses set up would tend to decrease its oblateness, and, the mass of the sphere remaining constant, any tensions set up would be parallel to a meridian and compressions parallel to the equator. This astronomical explanation is plausible, and suggests that meridian tension should cause a faulting more or less parallel to the equator, and equatorial compression create north and south ridges like the Andes and the Rocky Mountains. It is certainly consistent with the idea already alluded to—of an equatorial fault barrier which apparently interferes with megaseismic radiation meridionally. Another view is, that gravitational observations show that certain plateaux and mountain ranges are built of material of less density than those beneath the adjacent plains and ocean hollows; it follows that the lightest material has risen to the greatest height. Since denudation transports material from the high ground to the low, we can assume that the loading of the latter establishes a deep-seated plastic rock flow towards, but beneath, the denuded area, which is gradually lifted or buoyed up vertically. Assuming that such a process is intermittent, we can see in it a cause for sudden and violent adjustments of stress due to forces applied vertically. Various other theories, chiefly of a crypto-volcanic nature, have been advanced to explain these sudden strains on the earth's crust. They involve the action of variations in heat and pressure inside the earth's mass, leading to variations in volume of steam and gases conducive to violent adjustments for the relief of strain: similar to these are effects of percolation of water through fissures and subsequent evolution of steam by contact with molten rock. All these may be contributory factors towards earth-tremors, but, as agents in the production of earthquakes, they are probably of subordinate importance.

The ordinary theory of earth faults is that they are the result of a rigid

crust accommodating itself to a shrinking nucleus. As the nucleus of the globe contracts from the loss of heat, the superincumbent crust, unable to support its own weight, buckles, and is fractured. When this happens, compressional strain is relieved, and a mass displacement takes place. If this theory be correct, we should expect to find evidences of compression in an area affected by a megaseism. This has not invariably been the case. Other bathyseismic forces may be in the expansion or the contraction which takes place when certain substances pass from the fluid to the solid state, or after solidification whilst cooling. Most metals contract when they solidify or cool, but there are exceptions such as bismuth, and certain alloys of zinc and copper and of tin and copper. Another possible sequence of events may be that, instead of a shrinking nucleus retreating from an unsupported crust, there may be a shell suddenly and intermittently contracting, and thus tending to destroy itself by the tightness of its grip upon an unyielding mass beneath. If this be so, then earth-faults arise not only from compressional strain, but also under tensile strain. A strong argument in favour of the view of a contracting crust upon an unshrinking nucleus is that in areas of the greatest megaseismic activity there is evidence of the maximum loss of earth-heat, and therefore the cooling of whatever may lie beneath these areas will be most rapid. An examination of heat gradients in the earth's crust shows that near to and under the sea they are high, whilst at long distances from the oceans they are low—the loss of heat being proportional to the steepness of the gradient. It is noteworthy that the geographical distribution of megaseismic activity agrees closely with the geographical distribution of thermometric gradients. Under the oceans the gradient is roughly one degree Fahrenheit in twenty feet, while the continental gradient is one degree Fahrenheit in seventy-six feet. It is assumed from this that the earth's crust under deep oceans has either a higher conductivity for heat than that beneath continental areas, or else the crust is thinner. In either case it follows that under the oceans the crust would be most liable to sudden contractions following solidification. It is in these sub-oceanic areas that the origin of the majority of megaseisms appear to lie. Whether it be the case of a rigid crust accommodating itself to a shrinking nucleus, or an unstable shell fracturing itself by contractions and grips upon an unshrinking nucleus, we can understand how earthquakes arise, and also appreciate how the study of their records must gradually help towards a better understanding of the physical constitution of our globe.

ON THE INSTINCTIVE FACTOR IN HYSTERIA.

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(*Continued from p.* 121.)

The Relation of the Foregoing Views to the Current Theories of Hysteria.

The most popular theories of hysteria at the present time seem to be those (*a*) of Babinski, (*b*) of Janet, and (*c*) the Breuer-Freud-Jung theories. Of these the Babinski theory is the most easily grasped, and is attractive for its logical, simple and clear-cut nature. It is summarized by its author in his "Exposé des Travaux Scientifiques," published in 1913, and has been applied to the conditions of the war in the little book entitled "Hysterie, Pithiatisme, etc.," written in collaboration with Froment. According to this theory, "Hysteria is a pathological state manifesting itself by disorders which it is possible to reproduce by suggestion, in certain persons, with complete exactitude, and which can be made to disappear under the influence of persuasion (contra-suggestion) alone" [39]. The distinguished author goes on to deny that the stigmata exist if their presence is not suggested by the physician, and by numerous and careful observations he has shown that when such stigmata are sought in such a way that the patient receives no suggestion from the examination they are never found. As noted above he has made a great many investigations into such conditions as "hysterical" trophic and vasomotor disturbances, alterations of reflexes, etc., and concludes from these investigations that they are always due to organic disease or simulation. By way of proving that suggestion is not only a cause of hysteria but actually the *sine qua non*, he had many observations made on persons in states of intense emotion such as occurred in the Messina earthquake and in the mortuaries of Paris when the bodies of the dead were recognized by their relatives. As a result of these inquiries he ascertained that emotions alone never produce the symptoms of hysteria at the time they are experienced most acutely, and he concluded that "when sincere and profound emotion shakes the human soul there is no room for hysteria," but that the symptoms arise only by the intervention of suggested ideas in affective states of less intensity. There is no authority who has given so clear and consistent a picture of the disease as Babinski; and if the acceptance of his teaching means the circumscription of the scope of hysteria within narrower bounds, and the changing of its name to "pithiatism," that is in all probability nothing but an advantage.

Nobody with experience of functional nervous disorders is inclined to dispute the great influence of suggestion in determining their form. Epidemics of "tarantella dancing," of singultus, of fantastic immorality among women, of mutism in armies, etc., are all too well known, and are due to the power of suggestion on unstable intellects, just as the globus hystericus probably owes its origin to the difficulty in swallowing during weeping, and monoplegias are often the sequel to trivial injuries to the limb. But the question is whether the attribution of these things to suggestion is as far as we can go in explanation of them. If a man invests money in certain popular stocks we shall often be perfectly right if we say that he does so under the influence of suggestion, inasmuch as all his friends are buying the same things and suggesting the same course to him; but we can go at least a stage further back and attribute his action to the influence of the desire to live if he be a poor man, or to the parental instinct if he does it for the good of his children, or to the instinct of acquisition if he is a rich man already. So also we are perfectly justified in maintaining suggestion as the chief cause of hysteria; but the object of this thesis is to push the explanation a stage further back and to found it on the primitive instincts, especially those known as the self-regarding, and that of flight.

Janet's theory is rather more complicated and philosophical than Babinski's. He considers that hysteria belongs to the considerable group of diseases of the mind which arise from weakness and cerebral exhaustion, and of which the physical signs are somewhat vague and consist chiefly in a lack of general nutrition. The symptoms are mainly defects in morale accompanied by weakening of the power of mental synthesis, and an inertia of mind together with a narrowing of the field of consciousness showing itself in a special manner, viz.: a certain number of the psychic elements (sensations and images) fail to be perceived and seem to be kept outside the personal consciousness—the result of this is a tendency to complete and permanent schism of the personality into independent groups which may appear by turn in the focus of consciousness or even co-exist there. This imperfect synthesis of the elements favours the growth of certain "parasitic ideas" which develop completely free from the personal consciousness and only show their presence by a variety of disorders which appear as if they were entirely physical in their nature [40].

In short, the automatic association of ideas is one thing, whereas the synthesis which constitutes the personal perception at each moment of our life, and the very idea of self, is another. The latter can be destroyed while the former continues to exist [41].

Probably the only reason why this theory has not by now found its way into every textbook on medicine is that the notion of the doubling of a personality is slightly too speculative to appeal to the busy student or practitioner. It seems to cover the ground as well as any theory of so complex a phenomenon can be expected to do, and is applied by its author to an enormous number of observations made at the bedside. Against it is

that it is founded to a great extent on the results of interrogating patients. A woman says she has no recollection of certain behaviour and it is concluded that the elements of the mental state underlying that behaviour have not been synthetized. She says she has no knowledge of what is done (for example) to her right arm, or that objects in certain parts of her field of vision are invisible, or that food never excites her appetite, and we deduce that her consciousness is narrowed and that some of its contents have been separated. But why should we attach so much importance to these statements? The word of the neurotic person is surely a sandy foundation to build upon. It is quite true that nobody accepts such statements at their face value unless corroborated by the results of physical examination; but when the data obtainable are carefully examined this corroboration is generally found to be more apparent than real; for hysterical blindness is never so complete that the patient hurts himself in moving about, "astasia-abasia" does not result in serious falls, and an anæsthetic limb does not sustain any injuries more serious than those seen in artificial dermatitis. It seems unlikely, if hysteria were "a form of mental disaggregation characterized by the tendency to permanent and complete doubling of the personality" [42], that the affected members would be protected from injury by the action of the main personality.

However, if a splitting of the personality is to be considered as the explanation of hysteria, can it not equally well be held to be at the bottom of all instinctive acts? It frequently happens that a man driven by fear, hunger or love becomes to all appearance a different person and may have only an indistinct recollection of the deeds done under the excitement of the moment. The queen-bee in her nuptial flight is a totally different character from the inert, parasitic mother of the hive; the lapwing, which is among the most timid of birds, becomes tame and even aggressive when her parental instinct is aroused, and the stag not only changes his temper but his very form (by the growth of antlers) in order to follow the reproductive impulse. It would therefore appear that instinctive behaviour should come under Janet's description of hysteria. The reader, then, who feels inclined to agree with the theory advanced in these pages need not be deterred form doing so by the fact that he is an adherent of Janet; for the present hypothesis would form a sort of connecting link between the speculative and abstract requirements of the disaggregation theory and the proximate, practical explanation of Babinski.

In trying to state the relationship of any theory to that known as the Freudian, one is met by the difficulty that the latter is in a state of flux or evolution, and that what appeared fundamental yesterday may be greatly modified to-morrow. This, of course, is to be expected in the advance of new teaching, but it inevitably adds to the difficulty of the medical profession in comprehending it and assessing its value. Another stumbling-block to inquirers is the profuse (one might almost say reckless) employment of metaphor and analogy in the works of Freud. Notions of mental

trauma, of psychological catharsis, of resistances, of censors, of foreign bodies in consciousness, of layers of the unconscious, of repression, of displacement and diffusion of an affect, of bringing the unconscious idea into the light of day, of sublimation and other such phrases, arouse great uneasiness and distrust in many people who realize the numerous and subtle fallacies that accompany the use in science of the devices proper to poetry. Havelock Ellis has tried to justify this wealth of figurative language by assuming that the theme and material do not lend themselves to strictly scientific, nor yet to strictly metaphysical, treatment, but require an artist's outlook and an artist's methods [43]. If this is so, it explains a great deal of the opposition to the new doctrines shown by physicians accustomed to more exact studies.

It is, moreover, difficult to test the psycho-analytic system in practice, for to do so necessitates mastering, firstly, a very recondite code for the symbolic interpretation of dreams, and, secondly, the technique of very tedious "association methods," which require a high degree of skill and training for their proper use. Dreams are stated to have a latent as well as a manifest meaning, and always to illustrate a concealed wish. According to the code, to ask the way in a dream may signify Rome, for all roads lead to Rome; and the meeting of an acquaintance called Zucker is a veiled way of alluding to Carlsbad whither patients go with Zucker-Krankheit [44]. To hurl oneself into the water is an inversion for coming out of it, of which the latent meaning is to be born [45]. To change one's residence is equivalent to undressing [46]; while not only all cylindrical and all round or oval objects, but also a hat, a cravat, a church, an apron, a forest, a mountain, a box, a snake when occurring in dreams, signify the reproductive organs of one or other sex. Water, stairs, inclines, and many other things bear the latent meaning of copulation [47]. Numerals even, when dreamt of or mentioned by chance in conversation, have a latent meaning which is far removed from that which is usually accepted [48].

The drawbacks to discovering the patient's wishes by this method of dream-analysis are obvious enough, and are aggravated by the fact that different investigators will interpret the same dream in different senses. There are similar disadvantages in the use of the association methods. One of these, favoured by Freud, consists in inducing the patient to discourse with absolute freedom on any subject that presents itself to his consciousness. He must learn not to impose any check upon himself but to mention everything that enters his mind. From the content of these thoughts, together with the order and manner of their appearance, the analyst arrives at conclusions concerning the complex of memory and emotion which is at the root of the trouble. The Zurich school employs another method which consists in reading out test collections of words to the patient, and noting what associated ideas they bring to his mind, and how long the reaction takes to occur. The aim of the method is the same as that of free-association, namely, the exposure of the pathogenic complex.

It is claimed that when the complex has been uncovered or brought to light, its energy is diffused through the mind and so dissipated, and the patient is permanently cured [49]. The process of analysis may occupy six months or more.

The truth of the psycho-analytic postulates is held to be demonstrated by the success that attends the curative system, and since the application of this system involves so many serious difficulties, it is not easy to estimate how much of the theory is sufficiently established to warrant comparison with its rivals; so that it may be unfair to attempt any survey of it from the standpoint of biology. Yet the fact that Freud's hypothesis is founded on the instinct of sex indicates that the two theories start from common ground. They separate afterwards mainly because the psycho-analysts have tended to narrow their outlook until they regard sex as the dominant human impulse, whereas the biologist takes a wider and more comprehensive view of the springs of action, and relegates the sexual to its proper place among the other human instincts.

Jung, by abjuring some of the excesses emanating from Vienna, has greatly strengthened the influence of analytical psychology. His conception of the "libido" as not exclusively sexual desire, but practically the entire conative function of the mind [50], brings him within measurable distance of the central position of the biological school.

Note on Treatment as affected by Biological Considerations.

The treatment of hysteria has been successfully carried out in a great variety of ways. Janet [53] divides the available methods into groups: (*a*) Moral influence and the utilization of automatism; (*b*) psychological economies; (*c*) psychological acquisitions. Under the first of these come religious influences such as the cure at Lourdes, "Christian Science" and similar methods, suggestion and hypnotism. Under the second, rest cures, isolation cures and psycho-analytic methods. Under the third, the methods of re-education, stimulation and physiological adjustment.

Each of these methods has had its exponents who have practised their particular favourites to the exclusion of all others. The wisest physicians have made use of them all according to the requirements of circumstances and each individual case, and have frequently combined such as are not incompatible with each other. Though many teachers claim that their method produces a complete and permanent cure, it is doubtful if any treatment can remedy what used to be called the hysterical diathesis. The symptoms can be abolished and the patient brought to a healthier frame of mind, on her guard against the onslaughts of the "unconscious"; but when unfavourable conditions recur, it is very likely that she will again fall a victim, even if to a less degree than formerly. Patients who have undergone a very long and expensive cure are less likely to return to the same physician when they relapse than are those who have been cured

cito, tuto, et jucunde; hence the difficulty of judging the results of the more elaborate treatments.

If it is agreed that the origin of the symptoms is the exaggeration of an instinct, or the impulse of some thwarted instinct satisfying itself as best it can, the first step in treatment will be to ascertain what that instinct is. It may be positive self-feeling or negative self-feeling, or one of these combined with the sexual, or possibly the sexual alone, or flight.

In the case of the last the treatment is, of course, simple, and consists either in removing the cause of fear or in training that patient to tolerate it. If the symptoms persist after the cause of fear has been removed, they are due to the super-added influence of the instinct of display, and must be treated accordingly. When there is reason to believe that the trouble is due to exaggerated positive self-feeling alone as in the case of the child quoted who shouted offensive words for days on end, the cure can be easily attained by stimulating, unsympathetic means such as a cold douche, electricity or a spanking. For example, Private X., of the Royal Army Medical Corps, being stationed during the war in a safe place, suddenly became mute after reading an account of some "interesting" nervous cases. He came under the care of an impetuous Celtic officer, who, being overworked and irascible, seized him by the throat and exclaimed, "If you don't speak, you ——, I'll squeeze the life out of you." The cure was instantaneous and, as far as could be ascertained, permanent. But the best treatment for this class is undoubtedly isolation. It acts by removing all possibility of self-display, and when used in this particular type of case has achieved a very great reputation. It is less certain in its action, though often useful, when negative self-feeling is present; and it is quite out of place when a permanent and incurable cause of depression exists. In such cases it is likely to exaggerate the depression.

When an injury to self-esteem and the presence of negative self-feeling are at the root of a hysterical trouble the treatment is more difficult than in the foregoing class; but good results can be obtained if the self-abasement is caused by curable pain or by failure in some social activity such as often occurs in schools. In the case of pain it must, of course, be removed when possible; or if the pain is already past, as will often be the case, the origin of the trouble can be explained to the patient, and persuasion and encouragement will bring about the disappearance of the nervous phenomena. If maladaptation to the social surroundings is causing the hysteria, by far the best course is to change the environment before the abnormal nervous condition becomes habitual owing to the excitation of the instinct of self-display, in which case the symptoms may persist even in a new school or a new neighbourhood.

If injury to self-esteem is allied to injury to the reproductive instinct, as is frequently the case owing to the fact that the self-regarding instincts have one of their chief uses as adjuvants to sexual selection, the treatment will be further increased in difficulty. Fortunately injuries to the

reproductive system are often imaginary rather than real, and by convincing the patient of this a cure can be effected. For example, a young man, recently married, asked for operation for left inguinal hernia and undescended testicle. As the testicle was atrophied and useless the surgeon removed it at the operation. A short time after leaving hospital the man came back complaining of excruciating pain in the left iliac fossa and inguinal region. In the course of conversation it was discovered that he was afraid his virility would be impaired by the loss of the testicle. The pain disappeared when he was convinced that no appreciable impairment had occurred.

Young women who have been jilted or who have quarrelled with their lovers frequently exhibit states of aphonia, asthma, paresis, etc., which disappear when the quarrel is settled or another lover appears. These cases are so frequent as to be a commonplace of practice. They illustrate the milder hystero-genic effects of the instincts of reproduction and self-regard combined.

The cure of hysteria, and even it is alleged, of insanity, has been very often effected by surgical operations for the rectification of some visceral displacement or minor gynecological condition harmless in itself [51] [52]. There are three main types of such cases. Firstly, the symptoms may be merely a form of self-display not preceded by any cause of depression. Hysterical symptoms of this origin (as just stated) will not survive any unpleasant, stimulating treatment. The hardships of the operation therefore bring the patient to reason. Secondly the knowledge or discomfort of the displaced organ may be a constant source of negative self-feeling to the patient and the operation acts by making her "as good as other people." Thirdly, the patient may be suffering from some chronic cause of negative self-feeling such as an unhappy marriage, premature widowhood or disappointed hopes of any kind. For her the operation with its pomp and circumstance, of which she is the centre, produces such elation of spirit that its unpleasant features are disregarded and she is (temporarily, at least) cured of her hysterical troubles. This type often comes back again to the surgeon for operation and thrives on the abnormal stimulation of the nursing home. It must, however, be remembered that operations have a twofold action on the patient's mind, one stimulative and the other depressant. It is not easy to predict which of these is going to gain the ascendance in any case, and it consequently follows that an operation which has been confidently recommended on account of previous successes may only make a patient more hysterical than before.

Generally speaking, only the slighter operations should be allowed in hysteria, when choice is possible, as they are less likely to cause depression.

There is another kind of case whose nervous symptoms are neither cured nor aggravated by an operation but date from it. Such persons have the normal dislike of illness and operations, but yet on account of disease have to submit to some mutilation such as the removal of the

reproductive organs. In them the pathogenic influences of pain, fear, wounded self-esteem and loss of reproductive hope are combined, often with terrible effect. The object to be kept in mind in treating such cases is the diversion of the attention from the painful idea to others which are beneficial in their influence. Constant employment should be secured if possible and this should be of a nature that will minister to the patient's self-esteem. A woman may find alleviation in works of charity, in such occupations as amateur acting, singing in public, or even sport; a man may be interested in politics, military exercises, money-making, etc. In any case the pursuit should be one in which the patient can excel and stand out from his neighbours. The art of the physician consists in stimulating an already existing interest and in enlisting the help of the patient's friends in building up the sufferer's positive self-feeling, since he cannot restore the power of reproduction. A certain number of his patients will be too badly equipped by their previous habits of life ever to start afresh; and a certain number will have the temperament known to the French in war-time as "défaitiste"; some will have taken to drugs or alcohol. The best physician will be he who rescues the most of these. Two things which the treatment must not be allowed to do are to induce romantic relations between doctor and patient when of different sexes; nor must it minister to the former's self-esteem instead of to the patient's. For these reasons, and because sex and self-feeling are so closely allied, it would probably be an advantage if "neurotics" could always be treated by practitioners and nurses of the same sex as themselves. This may perhaps become customary in the future.

SUMMARY.

The springs of action are obscure and not to be discovered at a glance. The impulses that move animals are in the main identical with those that sway human behaviour, and are studied by biologists under the headings of reflex action, instinct, intelligence and reason. Of these reason takes but an infinitesimal part in animals' conduct, and a small but important one in that of man. If we take instinct as the central phenomenon and try to divide it from reflex action on the one side and intelligence and reason on the other, we find that though pure types of each are recognized, yet there are no lines of demarcation where we can say definitely that one ends and the other begins. In this respect the study of instinct is in the same case as that of morphology of species, but morphology has the advantage inasmuch as pure types of structure are much more common than unmixed motives of conduct. It is for this reason that the disorder of function known as hysteria which the biological theory claims to be due to the derangement or excess of instinctive action, is so difficult to differentiate from the disorder of reflex action which constitutes disease [54] and from the working of intellect which constitutes malingering.

Various instincts have been described by McDougall and other writers

as being observable in human conduct. Of these the only one that has attracted much attention (until lately) among physicians has been the sexual or reproductive instinct. The object of this article is to draw attention to the importance of other instincts, notably of flight and the self-regarding instincts, both as motives of human conduct in general and as causative factors of hysteria. An attempt is made to show that the reproductive and self-regarding instincts act very generally in combination (as parts of what McDougall calls the self-regarding sentiment) and that it is when they are so combined that they are capable of producing the greatest harm as agents of functional disease.

When the biological is compared with the theories propounded by Babinski, Janet and Freud respectively, it is found to be in general agreement with their basic ideas, and perhaps to form some sort of link between them.

AUTHORITIES QUOTED.

[1] DREVER. "Instinct in Man," pp. 18 and 19.
[2] Op. cit., p. 99.
[3] ROMANES. "Essay on the Darwinian Theory of Instinct."
[4] LLOYD MORGAN. "Animal Behaviour," Second Edition, p. 71.
[5] W. McDOUGALL. "Social Psychology," Tenth Edition, p. 29.
[6] PECKHAM. "Wasps," 1905, pp. 292, 293.
[7] STOUT. "Manual of Psychology," 1915, p. 343.
[8] FABRE. "The Sacred Beetle" (Trans. Texeira de Mattos).
[9] SHERRINGTON. "Integrative Action of the Nervous System," 1906, p. 250.
[10] JENNINGS. "Behaviour of Lower Organisms," 1915, p. 278.
[11] LLOYD MORGAN. "Animal Behaviour," p. 71.
[12] SHERRINGTON. *Op. cit.*, pp. 178, 179, and Ch. V generally.
[13] HEAD. *Brain*, vol. xli, p. 201.
[14] THOMSON, J. A. "Secrets of Animal Life," 1919, p. 173.
[15] Op. cit., p. 118.
[16] DARWIN. "Formation of Vegetable Mould," p. 98.
[17] FABRE. "Merveilles de l'Instinct chez les Insectes," 1919, Ch. V and VI.
[18] Op. cit., p. 301.
[19] HOBHOUSE. "Mind in Evolution," 1915, p. 97.
[20] LLOYD MORGAN. "Habit and Instinct," pp. 40-42.
[21] Op. cit., pp. 304, 305.
[22] BERGSON. "Evolution Créatrice," Fifth Edition, pp. 179-183.
[23] HOBHOUSE. Op. cit., pp. 178-180.
[24] Op. cit., Ch. XI.
[25] Op. cit., p. 49.
[26] CREIGHTON. "Life of Sir Walter Raleigh," p. 34.
[27] ATTERIDGE. "Marshal Murat," p. 202.
[28] Loc. cit., p. 65.
[29] FLAUBERT. "St. Julien l'Hospitalier."
[30] BABINSKI and FROMENT. "Hysteria or Pithiatism," *Trans.*, 1918, p. 17.
[31] PRINCE, MORTON. "The Unconscious," 1916, p. 247.
[32] RIVERS. "Instinct and the Unconscious," 1920, p. 16.
[33] BRISTOW. "Theory and Practice of Medicine," 1887, p. 1199.
[34] PITRES et LAFFAILLE. *Revue de Médecine*, 1920, Nos. 5, 7, 8.
[35] CAMERON, HECTOR. "The Nervous Child," 1919, p. 32.
[36] BABINSKI and FROMENT. Op. cit., Ch. II.
[37] HURST. "Psychology of the Special Senses," 1920, p. 40.

[38] Babinski. "Exposé des Travaux Scientifiques," 1913, p. 198.
[39] *Ibid.*, p. 203.
[40] Janet, P. "L'Etat Mental des Hysteriques," 1911, *passim*.
[41] *Idem.* "Automatisme Psychologique," 1889, p. 234.
[42] *Idem.* "L'Etat Mental," p. 447.
[43] Ellis, Havelock. "Essays in War Time," *Psycho-Analysis*, 1919.
[44] Freud. "The Interpretation of Dreams," Trans., 1920, p. 103.
[45] *Ibid.*, p. 243.
[46] *Ibid.*, p. 321.
[47] *Ibid.*, Ch. V.
[48] *Ibid.*, "Die Psychopathologie der Alltags Lebens," Ch. XII.
[49] Jones, Ernest. "Treatment of Neuroses," 1920, p. 127 *et seq.*
[50] Jung. "Collected Papers on Analytical Psychology," Constance Long's translation, 1917, pp. 260, 274, 347, 348, etc.
[51] Bossi. "Die Gynakologische Prophylaxe bei Wahnsinn," 1912.
[52] Suckling. "Movable Kidney," 1905.
[53] Janet. "Les Medications Psychologiques," 1919.
[54] Sir James Mackenzie. *Brit. Med. Journ*, 1921, i, p. 147.

OLD AGE AND BLOOD-PRESSURE PROBLEMS.[1]

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From the time that we began our closely collaborated work at the Royal Hospital, Chelsea, we have made frequent use of the Tykos blood-pressure apparatus—at first regarding it as a valuable means of estimating departures from the "normal," latterly finding it a most interesting support of our gradually dawning heresy that in dealing with old age there is no "normal." Taught that blood-pressure in the human being pursues an inevitable course, reaching a standard level about early manhood and gradually rising with advancing years, we attempted to correlate our findings with accepted teaching. We were soon puzzled by the paradoxical readings in men of over 75 whose radial arteries were nodular "pipe-stems"—one case exhibiting a pressure of 195/100; another, apparently parallel, 140/75; a third, 115/70. We then took systematic readings of 102 in-pensioners all from 75 to 92 years of age. The resulting figures are expressed in the graph which forms the basis of this paper. We do not attempt to lay down any law: we offer our figures, believing them to be accurate as far as instrumental accuracy may be relied on, to the many who are interested in blood-pressure readings.

We add a few remarks which have materialized from our heated discussions over points which have puzzled us, and, confession being good for the soul, puzzle us still after groping in the literature for a solution.

Principal Factors of Blood-pressure.

The principal factors concerned in blood-pressure are presumably: (1) the heart—the pump; (2) the arterial system—the pipes; (3) the capillary bed—the field of irrigation; (4) the quality of the blood—the irrigating fluid; (5) the quantity of blood available and (6) the mechanisms regulating the calibre of the pipes and the rate of blood-flow, nervous, chemical, toxic, mechanical.

Up to and including middle-age, the heart, the pump, in the absence of obvious endo- and myocardial disease, may be accepted as a constant factor; as may the arteries, the pipes, in the absence of obvious sclerotic change. Under such circumstances it has been possible to formulate—by compression of the left brachial artery—a standard, an average taken from many thousands of cases—in fact, a "normal" of pressures taken at this site of election. In the physiological laboratory it is impossible to model a pump comparable with the living human heart, still more impossible to devise

[1] Reprinted from the *Lancet*, 1922, ii, p. 503.

any contrivance to represent a degenerated heart, the action of which cannot be guaranteed from hour to hour, even from beat to beat.

The primary factor in the blood-pressure phenomenon is the heart, and we submit that it is impossible to formulate a blood-pressure "normal" for a period of life during which the heart is an inconstant factor. General, or localized, arterial disease again precludes the possibility of supposing a standard applicable to old people. We do not suggest that every man over 80 years of age has a degenerated heart or established disease of the arteries, but it may be presumed that after so many years of constant use they are the worse for wear. From our post-mortem observations at Chelsea we have reason to believe that the large majority of old people have deteriorated cardio-vascular systems. The quality and quantity of the blood are factors which we have not investigated.

The nervous, chemical, toxic, and mechanical factors regulating and modifying the calibre of the pipes, and the rate of flow of the blood, are subjects of which we *know* nothing. The graph shows that the majority of our 102 cases exhibit a mode of 130 to 169 millimetres systolic pressure, with a majority of eight cases at the lower level of 130 to 149. An undoubted preponderance, fifty-four out of 102 cases, with diastolic pressure from seventy to eighty-nine millimetres Hg occurs. Pulse pressures appear to vary mainly between fifty to eighty-nine millimetres Hg. But to apply these figures as a "normal" for this period of life is obviously impossible when one is faced with active die-hards with signs of neither hyper- nor hypotension, and no apparent answer to the puzzle that their blood-pressure readings vary from 190/100 to 95/45.

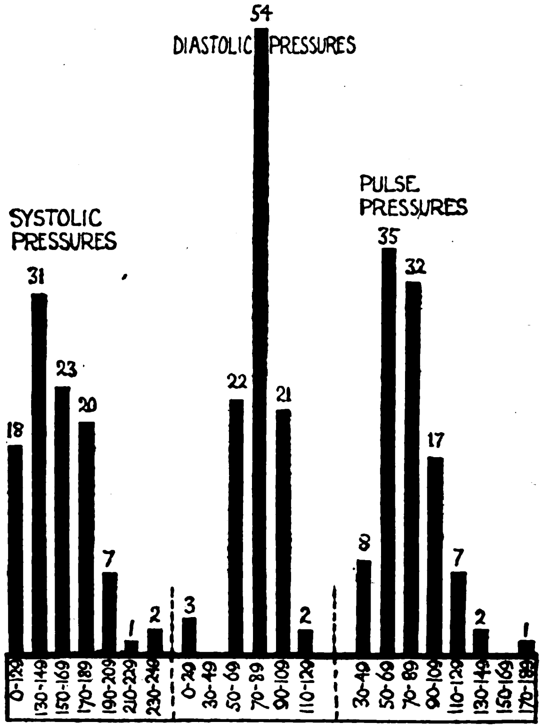
Abnormal Blood-pressure as a Morbid Symptom.

Rapid variations—e.g., the fall with hæmorrhage, the rise with perforation, are valuable diagnostic signs in enteric fever. Gross, more or less constant, variations from the "normal" are commonly associated with the "red granular" kidney and Addison's disease, but it is not perhaps sufficiently recognized that abnormal blood pressure may be only one of several symptoms and often the least important. It is sometimes a useful indicator in estimating the progress of the particular disease and the results of treatment. Categorically to deny, as some do, that blood-pressure having attained a certain level can change to a lower level, compatible with the well-being of the individual, is to discredit the extraordinary adaptation of which the system is known to be capable, and of which further evidence and proof are continually forthcoming as scientific research proceeds.

Our findings have compelled us to become agnostics so far as blood-pressure problems are concerned; there are so many clinical findings for which there is no satisfactory explanation. Take the part played by the kidney, for example. Granted that signs of disease may not be demonstrable until a large proportion of the kidney is out of action, at what stage

in the disorganization of renal tissue does the kidney take a part in the production of hypertension? To what extent do the conditions grouped together as nephritis come into the hyperpiesia picture?

We have referred to the heart and arterial system as being two of the blood-pressure factors which preclude the possibility of establishing a "normal" for old age. The capillary bed, the field of irrigation, is a field of controversy in which even the wisest men tread very gently, and we dare do no more than sit on the fence and throw into the field a few conjectures from our clinical observations. Our notes are founded so obviously



Graph, showing blood-pressures taken from 102 men, from 75 to over 90 years of age, at the Royal Hospital, Chelsea.

on the work and writings of such authorities as Clifford Allbutt, Rose Bradford, Herringham, Dale, Norris, Batty Shaw and others, that in this short paper written round our figures we do not attempt a bibliography.

Further study of primary hyperpiesia may mould facts out of theories. Histamine is a toxic body and a "depressor." In surgical shock normally closed irrigation beds are said to be opened, the volume of circulating fluid reduced, and the blood-pressure is lowered in consequence. It may be found that toxic "pressor" substances, either by contracting the general pipe system or by shutting off certain irrigation beds, increase the rate of

flow or the volume of fluid to be dispatched throughout the system, and so increase, even temporarily, the pressure in those areas remaining open for irrigation.

Red Granular and Arterio-sclerotic Kidney.

It seems to be generally agreed that the "red granular" kidney and the "arterio-sclerotic" kidney are histologically the same as far as the vascular elements are concerned. Lawrence discriminates: "The red granular kidney is entirely due to occlusive endarteritis of the small arterioles of the kidney, of the afferent vessels of the tuft, and of the capillaries of the tuft. The scarred kidney—i.e., the arterio-sclerotic kidney—is due to occlusive endarteritis of the larger interlobular arteries, or arteriæ arciformes."

Though we see many arterio-sclerotic kidneys from the period of life under review in this paper, we do not see the syndrome associated with the red granular kidney. In our experience, a man who has reached the "old age stratum" seems to have passed the era of angina pectoris, the red granular kidney, and the higher grades of hypertension. Out of five fatal cases of cerebral hæmorrhage in our series three were in men of over 75 and two were men of 65. Pipe-obstruction seems more common than pipe-breaking in the furred-up and more rigid pipes of the damaged machines we have dealt with. It seems a reasonable suggestion that the more early fatal red granular kidney may be a condition, toxic in character, primarily renal in origin, which works back along the arterial system to the heart; while the arterio-sclerotic kidney, as a distinct condition, may result from primary hypertension operating centrifugally from the heart. We have seen in younger men the red granular kidney with obvious left ventricular hypertrophy and the textbook post-mortem findings. We have seen many arterio-sclerotic kidneys with hearts very little, if at all, hypertrophied, and in many cases with the myocardium, particularly of the right ventricle, definitely degenerated and atrophied.

Our blood-pressure figures from a number of men who may be presumed to have arterio-sclerotic kidneys—or Councilman's "chronic atrophic nephropathy"—do not point to resistance in the renal capillary bed as being an unimpeachable factor in the production of hypertension. The outstanding fact is the similarity in the histological picture of the "red granular" and the "arterio-sclerotic" kidney as far as the vascular elements are concerned. It is certain that, wherever the starting point, whatever the cause, whatever the specific difference in effect between these differently named but similar conditions, an adaptation compatible with extended life can be arrived at between the weakened but willing heart and the damped-down furnace of old-age metabolism.

Variations of Readings.

We have found the old men very tractable in tolerating our blood-pressure examinations. For the most part they hold the arm to be

examined well relaxed and in an unstrained position. Even their original idea that the Tykos and a stethoscope meant some form of vaccination did not unduly agitate them. We have noticed that little spurts of temper, disagreements as to which man was next for examination, etc., caused readings higher than usual for that individual. The readings from these very old men undoubtedly fluctuate to a disconcerting extent from causes which might easily escape the consideration of an observer not accustomed to senescents *en masse.* Perhaps the most interesting variations have been observed in a series of forty-six men, aged 76 to 92 years, whose readings were taken, for no particular reason, between 5 and 7 p.m. on Sunday and Monday, May 21 and 22, 1922, on which days the shade temperature suddenly reached 84° to 86° F. We compared these readings with previous figures of the same men and found that this sudden introduction of summer had, we suppose, either by depressing the heart's action, or by dilating the pipes and irrigation field, or both, caused a remarkable fall of pressure in seventy-two per cent of the cases. These new figures were graphed in order of age against the previous readings, and still further convince us that there can be no dogmatic "normal."

Certain cases gave particularly interesting readings. A man, aged 78, whom from several previous observations we had recorded as 200/100 showed 165/75 on the Sunday evening. Another man, aged 89, showed 182/72 as compared with our previous record of 200/95. Thirty-three out of forty-six cases showed a fall in pressure, both systolic and diastolic, of at least ten millimetres Hg. The figures of three men corresponded with previous readings. In ten cases the systolic pressure was higher by five to twenty millimetres Hg, with a slight correspondence in rise of the diastolic pressure. These differences in pressure seem to indicate the necessity, when making a "normal" standard for any series of ages, of ensuring that the readings are made under parallel conditions, climatic and otherwise.

Possible Prognostic Value of Pulse Pressure.

There is a class of case in which we suggest that further observation may perhaps show the pulse pressure to be a valuable prognostic sign. A man, aged 82, clinically myocardial degeneration, was admitted to the infirmary with the failing heart syndrome, œdema of legs, dyspnœa, etc. Pulse pressure twenty-five to thirty millimetres until he began, presumably, to rebuild his myocardial reserve. He returned to his bunk in the main building with pulse pressure sixty-five millimetres, able and glad to do his own light jobs for himself. A similar case, aged 83, showed a pulse pressure of thirty-five to forty millimetres while still very ill, but this figure became sixty-five as he recovered and had turned the corner. The diastolic pressure remained fairly constant, the load remained the same, but the systolic pressure rose with the increase in force of beat. We infer that the pulse pressure may be an indicator as to whether the myocardium

is or is not taking unto itself a new lease of reserve power and can combat, reasonably for the individual, that important factor the "diastolic load."

Small Practical Value of Readings.

The net result of our clinical blood-pressure observations on very old people is that the readings are more of theoretical interest than useful in diagnosis or treatment. A man of over 80 with no subjective or objective signs whose mechanism has become adapted to a 200/100 pressure is *not* a case of hypertension to be dosed with iodides and nitrites, to be carefully dieted and subjected to balneo-therapy. His medical attendant may be thankful that the man has been fitted with what is probably a compensatory and beneficent high systolic pressure. He will be wise in not tampering with the individual in order to treat a diagnosis.

We submit that his, or her, high or low blood-pressure is not a subject to be discussed with the patient either directly or by suggestion. There are already too many people who quote their blood-pressure figures. It is enough that such people regard their "nerves" and intimacies of metabolism as topics of general conversational interest. Freedom from anxiety, business, domestic and personal, is therapeutically most important for the subject of the hypertension which requires treatment. Patients who brood over their own blood-pressure figures, or the fact that their doctor has told them that they "suffer from high blood-pressure," are very prone to become the subjects of a vicious circle in which the processes revolve the faster the more introspective the patient becomes.

We wish particularly to thank Sir Humphry Rolleston for his very kind and constant encouragement in our work at Chelsea, and for the invaluable advice he has given us in the preparation of our papers for publication.

X-RAY PROTECTION.

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The increasing value and consequent more general use of X-rays for diagnostic purposes, and also as a therapeutic agent, render it very necessary that the fundamental physical principles involved should be more generally understood. The reason for this is mainly that protective measures may always be intelligently employed when X-rays are being used. In medical X-ray practice, X-ray dangers may be regarded as threefold :—

(1) Danger from the actual beam of X-rays.
(2) Danger from the high tension electricity.
(3) Danger owing to inadequate ventilation.

Each of these sources of danger threatens the patient, and all who happen to be present in the X-ray room during exposures; especially those who are constantly employed in X-ray work. Further, the result of improper or careless use of the rays and neglect of due precautions may not, by any means, be immediately apparent. The effect may be cumulative and symptoms only appear when the trouble is very far advanced. On the other hand all these dangers may be considerably modified if not entirely eliminated, by carefully designed protective measures.

It is proposed to consider the question of X-ray protection under each of the foregoing headings separately and to explain in very general terms the source of the trouble and show how due precautions may be observed.

In the first place it is pointed out that all exposures in radiography should be reduced to a minimum consistent with the production of good radiographs. Long exposures mean that the patient is having a large dose of radiation, for the same reason careful attention should be paid to the diaphragm, the opening of which should be as small as possible. Screen work, i.e., visual X-ray examination, should be reduced as much as possible; and all work as far as requirements allow, should be done by means of radiographs.

Dangers from X-rays.

X-radiation is known to be a form of radiant energy exactly the same as light and radiant heat, the only difference being one of frequency, i.e., the wave lengths in the case of X-rays are very much shorter than those of heat and visible light. X-rays are produced when the speed of an electron is altered, and this is achieved in all X-ray tubes by stopping an electron stream (cathode rays) by means of a block of metal (target or anti-cathode); the method of producing this cathode stream being different

in the case of a "gas" or "ion" tube, and a "hot cathode" tube of which type the Coolidge is a familiar example.

When the cathode particles strike the target, electro-magnetic disturbances are set up which result in the short wave vibrations known as X-rays.

It is pointed out that not all the cathode particles will be stopped at the same rate and consequently the resulting waves will not all have the same length, hence it follows that the X-ray beam produced in any X-ray tube is made up of different wave lengths and is mixed or "heterogeneous."

The X-ray wave length is determined by the change of speed of the electron, the greater the change of speed the shorter the resulting wave length and the more penetrating the beam of X-rays.

The speed of the cathode beam when striking the target is determined by the strength of the electric field between the electrodes; therefore penetrating or hard X-rays are generated when the electrical pressure across the terminals of the tube is high; but as stated above, soft or long wave length X-rays are also produced at the same time.

The property of the rays which is chiefly utilized in medicine is their ability to penetrate different substances, and as I have said, this ability to penetrate increases with the shortness of the wave length. The penetrability of any substance by X-rays depends upon its atomic weight. The greater the atomic weight of a body the greater its capacity as an absorber of X-rays and the greater the difficulty of penetration. Similarly to light, X-rays travel in straight lines, and so when passing through a substance and encountering another substance of higher atomic weight increased absorption takes place, resulting in the usual shadowgraph.

Whenever X-rays are impinged upon a body, a certain proportion of the radiation is scattered both at the surfaces of that body and also internally. In medical practice it should be remembered that this scattered radiation tends to have a fogging effect on the plate. The effect of this phenomenon, in this respect, may be lessened to a certain extent by a careful technique and proper adjustment of the "hardness" of the rays (i.e., the voltage) to the particular radiograph required.

The question of secondary radiation does not come within the scope of this article as in general Army medical practice, the question of protection is not concerned with it.

It will be seen from the foregoing remarks that the danger from X-rays is entirely bound up in the X-ray tube itself as the *source* of the radiation and therefore it is the *tube* that must be "protected" in the first place and not the operator. If it were possible adequately to protect the tube in such a way that no radiation should escape, then obviously lead aprons, gloves, etc., would be entirely unnecessary—this, however, for many reasons is not possible but at the same time the principle to be kept in mind is that the *tube*, as the very fount of the trouble, must be carefully protected *as far as possible.*

It has been said that the absorbing capacity of a substance with regard to X-radiation depends upon its atomic weight, therefore, it is clear that for a protective material this is the one desirable quality and for this reason metallic lead is usually chosen: other materials which are commonly used for the purpose, invariably contain lead in more or less quantity.

It may be noticed here that generally speaking, about 3 millimetres of commercial "lead rubber" is equivalent in absorbing power to 1 millimetre of metallic lead, and similarly about 7 or 8 millimetres of "lead glass" is also equivalent to 1 millimetre of metallic lead.

The tube should be, as far as possible, surrounded by three millimetres of metallic lead or its equivalent; and this should afford very generous protection and should allow of any type of X-ray work being safely carried out. It is pointed out that care must be exercised in fixing the protective material and ample space must be allowed round the tube, otherwise sparking may occur from the material used to the wall of the tube, resulting in its puncture. X-ray tube boxes and holders are invariably fitted with an adjustable diaphragm and it is again emphasized that this opening should always be as small as possible.

Fluorescent screens are usually covered with a layer of lead glass, but from the figures quoted above it will be seen that this is not to be regarded as adequate protection. Radiation passes through the screen and by the very nature of its employment constitutes a real danger to the operator. Its use, therefore, as already stated, should be severely restricted. It is obvious also that palpation under X-rays is dangerous; especially as efficient palpation inhibits the use of protective gloves, here again this work should be restricted to the greatest possible extent.

There is one aspect of X-ray danger which is very often overlooked, namely the different physiological effects of a beam of X-rays. It has been said that every beam of X-rays is "heterogeneous." It is comparatively easy to absorb efficiently the soft or long wave component of the beam, but it is not easy, in fact it is impossible in any practical manner to absorb completely the hard or short wave component. The physiological effect of X-rays entirely depends upon the absorption of the rays by the tissues; and from what has been said it will be understood that the soft radiation will be absorbed in the skin and superficial tissues, consequently any effect will be apparent there and in a comparatively short time. This, however, is not the case with regard to the hard radiation which, being more penetrating, is absorbed by the deeper tissues and the results are infinitely more obscure. X-rays have been shown to have a serious effect on the blood and glands. These effects are certainly cumulative and being unaccompanied at the outset by obvious symptoms are much the most dangerous. The protection recommended above will certainly absorb a very large proportion of the total radiation, but at the same time, some will escape and that which does so is potentially a danger. These remarks

apply of course to tubes operated at high voltage. Where the voltage is low there will be no radiation of high penetration produced. In addition to the lead protection recommended, the operator should remain several feet from the tube during exposures in heavy radiography.

It is to be noticed that when radiographing a thick subject with a low spark gap, i.e., low voltage and even when employing a heavy current through the tube, the exposure will be long and thus the risk to the patient of skin trouble is very much increased. Hence the need for adequate training and careful technique on the part of the operator.

X-rays as a therapeutic agent are becoming more widely recognized. The attendant risks in this work are considerable both for the patient and the operator.

Apropos of ringworm treatment, it must be remembered that X-rays have a deleterious effect upon hair follicles and unsuitable treatment may result in complete loss of hair. X-ray treatment should never be undertaken except by a trained and experienced radiologist and then only with extreme precaution, and the dosage should be carefully measured. It should never be undertaken unless suitable apparatus is available, i.e., filters and devices for measuring intensity.

Dangers from High Tension Electricity.

High tension is necessarily *always* employed in an X-ray installation. The high voltage necessary to give acceleration to the cathode stream is usually provided by means of either an induction coil (Ruhmkorff coil) in which the primary current is made and broken mechanically, or by a closed core static transformer.

To consider in the first place the induction coil. This consists of an open core static transformer and used with it is a device for interrupting the primary current. This is the most general type of installation found in military hospitals and is admirably suited to the work that is usually required. In addition to outlining the precautions which must be adopted in the use of all very high voltages, it would be as well perhaps to say a word or two here about the general care and operation of an induction coil. The proper performance of a coil depends very largely on the type and efficiency of the "interrupter" which is used to make and break the current. The modern type of interrupter in common use consists essentially of a jet of mercury rapidly rotated in a dielectric of coal gas by means of an electric motor and so making a series of intermittent electrical contacts. Perhaps the most common cause of failure of coil outfits is traceable to the indifferent performance of the interrupter. This instrument should be periodically taken to pieces and cleaned and the mercury channels kept free; the mercury should frequently be taken out and filtered before replacing. A condenser is also supplied with every coil equipment, and connected across the terminals of the interrupter in the

primary circuit. The condenser should prevent any arcing in the interrupter. Induction coils produce inverse current, i.e., current which endeavours to pass in the wrong direction through the X-ray tube, and in order to "cut out" this current it is necessary to employ a special device. This usually takes the form of a commutating vane mounted on the top of the interrupter and rotated by the motor—the device works well as a general rule, its chief disadvantage from our point of view being that it occasions sparking at each contact. This sparking is a thing which it is desired to eliminate for reasons that will be given later; if, therefore, a rectifier of this type is used, it is desirable that it shall be enclosed in a cupboard.

Another method which is often adopted in order to "cut out" the inverse current is the use of "valve" tubes.

The valve tube is essentially a vacuum tube which is designed to have a low electrical resistance in one direction and a high resistance in the other. The use of suitable "valve" tubes is a highly efficient method of suppressing "inverse," but it requires constant care, and a certain amount of technical skill is necessary to operate the valves to the best advantage.

To consider now the question of the high tension closed core transformer. Insulation in this instrument may be effected either by air (dry) or the coils may be immersed in oil. It is worked directly by alternating current (usually single phase). There is considerable difference between these two forms of high potential generator, the chief being in the resulting voltage wave form, which has a very important bearing, not only on the resulting X-rays, but also on the life of the X-ray tube. The chief point of difference from our present point of view is one of power. It must be very thoroughly understood that the power of a closed core transformer is infinitely greater than that of an induction coil and the danger, therefore, is very much enhanced.

Both types of generator give "inverse" current and similar methods are employed in each case for its suppression. The method usually adopted in the case of the closed core transformer is to employ a synchronously rotating commutator—here again the resultant sparking is a drawback and the rectification is therefore usually done in a closed cupboard.

The most generally understood electrical danger is shock; although in the case of an induction coil shock is not likely to have fatal effect, it may cause serious discomfort, and it is often a cause of fear in uneducated persons, who inevitably associate the idea of X-rays with a very unpleasant electric shock and there is constant expectation during exposure of worse to come. It is in order to eliminate all risk of shock that a general earthing of *all* metal parts of an X-ray installation is recommended. This applies to all the metal work of the couch, diascope, and in fact all metal fittings. The earth connexion should be robust and well marked, and should be securely connected to a "good earth," e.g., a water main.

This general recommendation is not altogether an unmixed blessing, for example, a person touching an earthed part of the apparatus and at the

same time a high tension live wire will get the whole discharge through his body to earth with at the best, serious results; but if instead of an induction coil we have for the source of the current a high tension closed core transformer the *greater power* of the latter, under these circumstances, may result in death. Such circumstances should be impossible of realization in any well-designed installation, but at the same time, their possibility must not be overlooked. The obvious precaution is to always have the high tension leads as remote as possible. As an additional precaution against shocks to the operator it is recommended that rubber matting should be provided around all X-ray couches and screening stands.

The second danger to be apprehended from the electrical part of the installation is that of sparking, which will be dealt with under the next heading but it should be noted here that sparking should always be reduced to a minimum. This may be done by arranging all high tension leads and terminals in such a way that there are no sharp angles or loose ends which encourage corona. In addition all high tension leads should be round in section and as big as possible in diameter. All loose or even slack wires must be avoided. The main overhead leads should be metallic tubes of half or three-quarter inch diameter and spring leads also *round* in section should be used for other connexions. Apparatus should be arranged in such a way that all high tension leads may be as short as possible.

VENTILATION.

Artificial ventilation of X-ray rooms is recommended. The air should if possible be changed at least once every three minutes during screening and in fact whenever the X-ray installation is operated.

The number of persons present in the X-ray room during operation should in all cases be reduced to a minimum.

It has been stated that corona discharge (sparking) should be reduced as much as possible by adopting certain general precautions, but with modern high power sets it is impossible entirely to suppress it. The result of corona discharge in the atmosphere is the production of ozone and nitrous fumes which have probably a deleterious effect on the occupants of the room especially if they are there for long periods of time. There is yet another possibility of danger, the effects of which may be much more obscure and may cause some physiological action due to ionization. It will be remembered that the whole of the atmosphere of an X-ray room becomes ionized during the operation of the installation and many of the headaches and other symptoms so frequently complained of by workers may be due to this phenomenon. These symptoms may be associated with the inhalation of the ionized air, or on the other hand may be due to a more direct electrical effect, but certain it is that complaints of this nature are very much reduced when ventilation is forced and efficient.

Enough has been said to indicate the general safety precautions to be observed in ordinary hospital radiography and also, while not attempting to treat at all seriously of the physics of X-rays, at the same time to show very roughly the reasons underlying the safety measures recommended. The writer had the privilege recently of inspecting the X-ray installations in the Military Hospitals and the foregoing remarks are based very largely on his experience of the points likely to prove of more general interest to the ordinary medical officer, who though not trained as a radiologist, is very often called upon to undertake some radiological work and who may wish to understand a little more concerning the physical aspect of his apparatus.

DISORDERS OF ENDOCRINE FUNCTION.

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The subject of dyscrinism is much to the fore in medical literature, and much has been written and discarded on this complex subject. Certain tests of definite value have now been obtained, and it will perhaps be of use to readers of the Journal to summarize from recent medical literature those of simple technique which require no special laboratory apparatus. If one may generalize from the numbers of cases occurring in the London district, dyscrinism in its minor forms must be exceedingly common throughout the Army, and no doubt many cases of slight functional nervous disorder and of D.A.H. depend upon such conditions which are amenable to treatment upon correct lines, but which are entirely unaffected by ordinary therapeutic measures.

(A) The Thyroid.

The thyroid-pituitary-adrenal groups are interdependent and in the main may be said to antagonize or balance the pancreas—a fact of some importance in proportioning the value of the tests to be described.

The test *par excellence* for thyroid function is that of the estimation of the basal metabolic rate which is by itself sufficient and requires no corroboration. The estimation of basal metabolism, however, requires elaborate apparatus not available except in well-equipped laboratories such as those of the Royal Army Medical College, London.

(1) *Hyperthyroidism.*

The symptoms of Graves' disease need no recapitulation and the diagnosis is a simple matter. Less definite overaction of the thyroid gland may be difficult to determine, but the following simple tests which can be done in any hospital should serve to demonstrate the condition.

(*a*) *Loewe's Mydriatic Pupillary Reaction.*—Originally, and still a test of pancreatic disease or defect, this test may be positive also in *relative* pancreatic deficiency, or in other words, in positive overaction of the thyroid-adrenal group. When obtained this phenomenon is of importance and should direct attention to pancreatic function. If pancreatic function is found, by additional tests, to be normal, then it is probable that overaction of the thyroid-adrenal group is present.

Method.—One drop of a 1 in 1,000 solution of adrenalin chloride (fresh) is put into one eye and is followed five minutes later by a second drop. A positive result will show as follows:—

(1) Dilatation of the pupil tested—present in one hour and lasting eighteen to twenty hours.

(2) Loss of myopic reaction in the tested pupil, on accommodating vision to near objects.

(3) No alteration in light reflex.

The result cannot be obtained a second time for several weeks at least.

(*b*) *Goetsch's Test.*—This consists of the intradermal or subcutaneous injection of five to eight minims of a 1 in 1,000 solution of adrenalin chloride. A positive result will show :—

(1) A central blanched area surrounding the point of injection, with a red areola in a few minutes.

(2) In half an hour the central area becomes lilac or lavender.

(3) In one and a half hours the areola becomes lavender or lilac, while the central area fades. The areola retains its lilac colour for about four hours.

(4) If tremor, nervousness, or tachycardia has been noted as a symptom of the case it will be definitely *increased in intensity during the half hour following injection.* Emotionalism may occur, but is not always a reliable symptom.

(*c*) *The Quinine Tolerance Test.*—Quinine hydrobromide is administered in 10-grain doses up to 50 or 60 grains in the day.

Cinchonism will develop in a normal man after 10 to 20 grains, but in hyperthyroidism much larger amounts can be administered without producing tinnitus or deafness, and 40 to 50 grains daily may be tolerated for weeks without producing symptoms.

(*d*) *Harrower's Thyroid Feeding Test.*—This test, details of which will be found under hypothyroidism is not to be lightly undertaken in cases of suspected overaction of the thyroid. There are, however, occasional cases of tachycardia accompanying underaction of the thyroid which may simulate the opposite condition and in these it may prove of value.

(*e*) Other tests of value are available, but they are of more elaborate technique. Chief in importance is the estimation of the blood sugar content after glucose feeding.

Following ingestion of 100 grains of glucose the resulting blood sugar curve in definite hyperthyroidism will approximate that of the diabetic type, i.e., there will be delay in the fall of blood sugar beyond one and a half to two hours following the dose; while in some cases the blood sugar may rise above the renal leak point and actual glycosuria result. If the blood sugar itself cannot be estimated the urine may be tested for sugar at half-hourly intervals after the glucose is given.

Apart from the symptoms and signs of established Graves' disease, such as thyroid enlargement, exophthalmos, etc., the most important signs pointing to hyperthyroidism in its minor degrees are :—

Disorder of the pulse-rate, tachycardia — this is outstandingly the most constant and important symptom—"cardiac excitability," best describes the condition. The heart is easily excited to a heaving or pounding action; with this there will be subjective sensations of throbbing

in the head or veins of the neck. Associated with this a degree of sympatheticotonia is nearly always present resulting in general nervousness, anxiety or tremor, also accompanied frequently by tendency to over-action of the secretory sweat glands. These symptoms may be present without any of the ocular signs of Graves' disease and possibly with but little thyroid enlargement, so that only definite tests will refer such symptoms to their proper category of hyperthyroidism.

Blood-pressure as a test is of little value. In Graves' disease there are three phases of varying pressure—an initial hypertension, a long period of hypertension, and a terminal stage of hypotension. Alterations of weight should always be recorded.

(2) *Hypothyroidism.*

Here again the most satisfactory test is that of the basal metabolic rate. Where this cannot be determined much may be learned from a study of the commoner clinical signs and symptoms in conjunction with Harrower's thyroid feeding test, which is done as follows :—

(1) The patient is confined to bed and the pulse-rate noted at four-hourly intervals during the day.

(2) On the next day (first day of test) he is given ½ grain of powdered thyroid, in a capsule, at 8, 10, 12 and 9 o'clock. The pulse is taken and recorded at 9, 12, 3, 6 and 9 o'clock.

(3) On the second day this routine is repeated using one-grain doses of thyroid.

(4) On the third day the same routine with two-grain doses of thyroid.

For convenience of estimation the pulse-rates should be charted and a curve drawn. If dyspnœa, twitching of fingers and eyelids, irritability or severe tachycardia develop during the test the thyroid should be stopped but the pulse chart should be completed for the three days of the test and for two further days making five days in all.

The results may be summarized as follows :—

(1) The normal man begins with a normal pulse-rate which is raised during the second or third days but which falls again to normal on the fourth or fifth, i.e., after withdrawal of thyroid medication.

(2) The hypothyroid case will show little or no response in pulse-rate.

(3) The hyperthyroid case will begin with a raised pulse-rate which will continue to rise and will remain high even on the fifth day because his thyroid gland is oversensitive to thyroid substance and *continues* to react after thyroid medication is stopped.

This test is most useful in the diagnosis of minor degrees of hypothyroidism, but must be closely watched if used for the opposing condition, in which it should rarely be required.

The main clinical signs and symptoms are summarized by Hertoghe and Harrower as follows :—

(1) Enuresis and polyuria—accompanied by, and said to be caused by,

excessive desquamation of the bladder epithelium which can be demonstrated in the urine.

(2) Dry harsh skin with excessive desquamation.

(3) Thinning of the outer half of the eyebrows.

(4) Dry and brittle nails and hair.

(5) Infiltrations in many regions, notably: subcutaneous tissues, periarticular tissues, ligaments—giving rise to laxity, subluxations, lordosis, flat-foot, painful heel.

(6) Mental symptoms: headache, giddiness and somnolence, with difficulty in expressing ideas and impaired memory.

(7) Various dermatoses, including such chronic affection as psoriasis, chronic acne and eczematous conditions.

To the above list one may add that investigation of thyroid function in cases of neurasthenia associated with varying degrees of enteroptosis and accompanied by subjective sensations referred to the pericardium will almost certainly repay the trouble of systematic tests.

This subject will be again referred to when other endocrine syndromes are discussed. One further point of interest arises from a consideration of Jobling's work on the mechanism of the therapeutic effects of iodides. This worker showed that iodides act only by stimulation of the thyroid gland, and that the absorption of chronic inflammatory lesions, such as gummata, is effected by neutralization of an unsaturated fatty acid which is produced by toxins, and which, until saturated by the addition of an iodine molecule, has the effect of inhibiting the solvent action of the lymphoproteases derived from the lymphocytes. In view of this work, the question of thyroid deficiency in all chronic inflammatory processes becomes of great importance.

The Adrenals.

Hyperadrenia is the term given to the effects of increased secretion of the adrenal glands. The resulting state is one of general exaltation of the thyroid-adrenal groups, which are interdependent. A state of increased excitability of the sympathetic nervous system—sympatheticotonia—results and the main symptoms are an increased metabolic rate, a raised blood-pressure and a general nervous excitability exactly resembling that seen in hyperthyroidism.

As Cannon and other workers have shown, adrenal stimulation is certainly produced by sudden emotion, while, on the other hand, all the symptoms of Graves' disease can be rapidly produced in animals by transplanting the proximal end of the divided phrenic nerve into the distal end of the divided cervical sympathetic, thus producing rhythmic stimulation of the sympathetic system at the respiration rate.

Harrower points out that adrenalin is rapidly destroyed when liberated into the circulation, so that its effects in conditions of worry and excitement are transient, and that this phase of excitability of the glands is followed by one of compensatory exhaustion when the opposite clinical feature is

obtained. This view corresponds closely with that of Kinnier Wilson, who finds no essential difference between the asthenia of Addison's disease and the asthenia of neurasthenia. This conception of the part played by the adrenals renders the symptoms of exhaustion during convalescence from any illness easily explainable under the term hypoadrenia or adrenal exhaustion—such a condition being merely a mild and prolonged degree of the acute hypoadrenia suggested by Crile in his theory of shock. Another term for it is that of vagotonia, a relative dominance of the vagus over the sympathetic (Purves Stewart).

The clinical symptoms of hypoadrenia are: fatigue—the outstanding symptom; added to this there may be bradycardia, low-blood pressure, coldness of extremities, urticarial eruptions, and mentally a lack of self-confidence and a depression of the processes of judgment.

In testing for adrenal dyscrinism it must be remembered that the thyroid function also is closely related to it and the functions of the two glands are nearly always simultaneously deranged, so that differentiation is often impossible. The tests for hyperadrenia are not simple and consist of:—

(1) Testing the rise of blood sugar and the urine for sugar after injection of approximately 1 c.c. of 1 in 1,000 adrenalin chloride.

(2) The oculo-cardiac reflex test. Pressure on the eyeball stimulates the vagus and causes slowing of heart. This reflex occurs in the normal but is much decreased in effect or is absent in hyperadrenia. In hypoadrenia, i.e., vagotonia, it *should not be employed* owing to danger of vagus stimulation causing cardiac inhibition.

Test for Hypoadrenia: (1) The white line test.

The abdomen is exposed for twenty minutes and at the end of that time a square or triangle is drawn on the skin, using a hard and blunt or rounded point. In half a minute the figure stands out as a white line which increases in distinctness and in breadth for two or three minutes and then fades. This reaction is said to be present in scarlet fever almost constantly and there are other fallacies which detract from its value.

It will be noted how greatly adrenalin figures in all the tests for the thryoid-adrenal group, a fact which merely emphasizes the close inter-relationship of these endocrines.

The following table of symptoms of adrenal deficiency is given by Harrower, who considers that any two are together sufficient to establish a probable diagnosis:—

(1) Asthenia—the fatigue syndrome with muscular and mental inefficiency—lack of energy.

(2) Low blood-pressure with a weak cardiac action and slow pulse.

(3) Subnormal temperature (presumably persistent).

(4) Malnutrition due to accumulation of waste products—deficient oxidation, etc.

(5) Acidosis.

Obviously the above claim presupposes the exclusion of any other discoverable causes for the symptoms mentioned.

Pancreatic Deficiency.

The many elaborate tests for pancreatic function which have been devised from time to time have all in turn been discarded, and reliance is no longer placed upon such tests as those of Cammidge, Sahli and others. There remain, however, three useful tests, the relative importance of which is summarized by Robert Hutchison as follows :—

(1) Loewe's mydriatic test (as described under the thyroid section).
(2) The sugar tolerance test also mentioned under thyroid section.
(3) The estimation of urinary diastase.

The details of this estimation (which can be made in two hours in any laboratory) are contained in various textbooks, and are therefore omitted.

The test was originally introduced to estimate loss of renal efficiency but is now mainly used for pancreatic function. In renal disease the diastase is reduced, in pancreatic disease it is much increased. The normal limits vary between six and twenty-five units in terms of starch-reducing capacity.

If the above three tests are all positive then the diagnosis of pancreatic deficiency or disease is probable—if to this probability there can be added the positive presence of creatorrhœa and steatorrhœa the diagnosis becomes certain. Expressed in a table pancreatic tests are :—

$$\left.\begin{array}{l}\text{(i) Loewe } + \ \ldots\ \ldots \\ \text{(ii) Sugar test } + \ \ldots\ \ldots \\ \text{(iii) Urinary diastase } + + \end{array}\right\} = \text{Probability of disease.}$$

$$\left.\begin{array}{l} + \text{ Creatorrhœa} \\ + \text{ Steatorrhœa} \end{array}\right\} = \text{Certainty.}$$

Pituitary Disorders.

The account which has been given of some of the commoner endocrine disorders is necessarily incomplete without some reference to the pituitary gland. The scope of this article, for which no originality is claimed, is, however, intended to be limited to those endocrine disorders which are liable to be met with in serving soldiers. It is, therefore, beyond its province to discuss the various manifestations of pituitary dyscrinism. Gross pituitary excess as also defect declares itself in unmistakable signs, and is not met with in the Army apart from occasional new growths such as gummata in the cranial cavity. The tests for simple dysfunction are all dependent on raised or lowered sugar tolerance. Different sugars may be given in varying amounts, and the blood or urine tested for these. In hypopituitarism there is marked increase in tolerance—in hyperpituitarism the reverse. It does not seem to be of practical use to discuss this subject further here.

On the subject of the gonads and their relation to other endocrines there has been much speculation. Sexual competence is an indication of well-being in the normal subject. Apart from the special aspects of ovarian dysfunction the matter is not one which concerns the Service medical officer. Occasionally bad habits in the young soldier may lead to disorders of digestion, tachycardia and functional nervous manifestations. The mechanism by which these symptoms arise is held to be through the disorder of the general endocrine balance which seems to be upset by an excessive exhaustion of the gonads and their internal secretions reacting on the thyroid-adrenal group.

REFERENCES.

McCallum. "Pathology," 1921.
Harrower. "Organotherapy," 1922.
Hutchison, Robert. *Medical Annual*, 1922.
Brown, Langdon. *Medical Annual*, 1922.

Clinical and other Notes.

AN UNUSUAL CASE OF APPENDICITIS.

By Captain D. C. SCOTT.
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Private A., aged 19, was admitted to the British Station Hospital, Secunderabad, complaining of fever and some abdominal pain. Unfortunately before I saw him he had been given ol. ricini. His temperature was 102·8° F. and pulse 80. He was admitted at about 8 o'clock in the evening and seen by me at about 12 o'clock. He had walked up to the hospital.

He complained of fairly acute pain localized in the right iliac fossa and the abdomen was moderately rigid. On palpation a marked tenderness was noticed over the right iliac fossa. His tongue was dirty. His pulse was of good volume and regular.

I diagnosed appendicitis, but as his temperature and pulse were inclined to come down, then being 101·8° F. and 76 respectively, I decided to have him watched and any change reported to me.

I saw him again at 7 o'clock and his condition was more acute. His temperature had risen to 103° F., pulse 84 and the abdomen was very rigid. His bowels had been open.

I decided to operate at once. The rectus incision was made and on opening the peritoneum the appendix presented at the wound. It was greatly enlarged and was gangrenous at the end. It was removed and as there was no sign of peritonitis the abdomen was closed.

He complained of a good deal of flatulence and abdominal distension that evening and the temperature which had dropped to 101° F. after the operation rose to 102° F. A flatus tube was passed and drachm doses of oil of cinnamon in water were given with some relief. He was given morphia, ¼ grain, at 9·30 and had a fair night, but in the early morning his bowels were open five times.

Seen at 8.30 his temperature had risen to 103° F., the pulse was of good volume and regular and 76 to the minute. He was complaining of abdominal distension but on examination the abdomen was found to be quite flaccid and there was no sign of peritonitis. The stools were sent to the laboratory for examination and a report was returned that evening that *Entamœba histolytica* and blood were present. Emetine, ½ grain, and morphia, ¼ grain, were given hypodermically and the patient passed a very good night.

Next morning I found him sitting propped up in the Fowler position, reading a paper and quite free from pain with a temperature of 99° F. Emetine, 1 grain per diem, was continued for twelve days and he made a complete recovery.

I had kept the appendix unopened in spirit and it struck me that perhaps I might find *Entamœba histolytica* in the appendix, so I opened it and to my astonishment found a date-stone impacted in the end which had evidently caused the gangrenous appendicitis. I then took a scraping of the stone and on examination under the microscope entamœbæ were found.

I then asked the patient if he had been in the habit of eating fruit and he stated that he had been accustomed to buy oranges and bananas from a fruit hawker, and on asking if he had eaten any other fruit he stated that thirty-six hours before coming into hospital he had bought some dates from this man and had eaten them, but he did not remember having swallowed a stone.

This case appears interesting for more than one reason. First it was very lucky that the diagnosis of appendicitis was made before the bacteriological report of dysentery was returned, as otherwise the acute condition might have been missed until perforation had occurred, as I have on several occasions seen cases of amœbic dysentery simulating acute appendicitis, and on one occasion it so simulated it that after consulting with three others, I operated and found a normal appendix and next day amœbæ were found in the stools.

Secondly one has continually heard it stated, and it is a very general idea in the lay mind, that appendicitis is caused by impaction of a pip or stone, but I have never actually seen it before.

Thirdly, that the eating of dates, which of all fruits in the East are most liable to infection, should have caused both the dysentery and the appendicitis.

In conclusion my thanks are due to Colonel Jack Powell, D.S.O., commanding the British Station Hospital, Secunderabad, for permission to publish this case, and to Captain T. O. Thompson, R.A.M.C., for the bacteriological examination.

A TRIP BY AIR FOR A SURGICAL EMERGENCY.

By Captain D. McKELVEY, M.C., M.D.
Royal Army Medical Corps.

On September 16, 1922, about 19.00 hours, I received instructions from General Headquarters, B.T.E., that I was to proceed the following morning at daybreak to Sollum, for the purpose of seeing a case of acute appendicitis. I was further instructed to take with me one nursing orderly and the necessary equipment for operating at Sollum, should this be found necessary.

Transport had been arranged by air from Helouan, and I was to report to the aerodrome there that evening.

The necessary dressings, towels, etc., were hurriedly selected, packed into two drums and sterilized. The instruments, ligatures, drainage tubes and anæsthetic apparatus were packed in an attaché case.

We eventually left Cairo at 22.30 hours, and reached Helouan aerodrome about an hour later. We were accommodated for the night at the aerodrome, and all arrangements were made for an early start the following morning in two D.H.9.a. machines. The journey was to be made via Aboukir—an Air Force Depot outside Alexandria—for the purpose of collecting a special form of stretcher on which the patient could be brought back by air to Alexandria, if it were considered advisable.

The morning of the 17th turned out to be foggy and by no means an ideal morning for flying. After some minor troubles we left the ground at 07.25 hours. The distance from Helouan to Aboukir is somewhere about one hundred miles.

Owing, however, to the condition of the weather, we drifted slightly off our course, and did not reach Aboukir till 09.15 hours, which was considerably more than the average time for this journey. On arrival at Aboukir we were disappointed to find that owing to some misunderstanding the stretcher we were to collect was not there, and that one would have to be made. An improvised stretcher, which consisted of a canvas jacket supported by a framework of Gooche splinting with the necessary straps for fixing it along the fuselage of the machine, was made within an hour and a half. We were again ready to start at 11.30 hours. Unfortunately the fates were still against us, as one of our machines could not be got to work, and eventually most of the sparking plugs had to be taken out and cleaned. We actually left at 13.00 hours. The weather had now completely changed for the better, and everything seemed to be in our favour.

The journey to Sollum was very pleasant, but apparently the R.A.M.C. orderly found it somewhat monotonous, as the pilot with whom he was travelling on looking back to see how he was getting on found him fast asleep.

We followed the Mediterranean coast westwards for a distance of 350 miles, with nothing to break the monotony of desert on one side and sea on the other, except a few Arab encampments, which appeared below us as tiny specks on the sand. We reached Sollum at 16.20 hours, having done this part of the journey in record time. I subsequently learnt that the generator of the machine on which I was had burned out one hundred miles from Sollum, and that we had travelled the remaining distance on accumulators. The pilot, however, fearing to alarm me, told me nothing about this until we had safely landed.

Sollum is an outpost in the western desert bordering on Tripoli. The only British troops there are a section of the Armoured Cars, which occupy an old Turkish fort on the top of the cliffs. From here desert patrols sally forth now and then to quell disturbances in the neighbourhood. On the plains below the fort is a small native village and the headquarters of the Frontier Districts Administration of the Western Desert. Provisions, drinking water and mails for the garrison are sent by sea from Alexandria once a fortnight. Other communication with civilization is by means of telegraph and wireless.

On arrival at Sollum I proceeded into the fort to see the patient, and found that he was suffering from acute appendicitis and general peritonitis. I decided that immediate operation was imperative, and at once set about to look for a suitable place in which to operate. I eventually found a disused shoemakers' shop in the fort, which I had washed out with cresol and rendered as clean as possible.

Fortunately an electric light generator had recently been installed in the fort, and it only remained to run a cable into the improvised operating theatre and fix the necessary bulb. Fortunately there was a good reserve supply of fresh water at hand.

At 18.45 hours, that is to say, just over two hours after arrival, the operation commenced—350 miles from civilization and under conditions which would compare very favourably with anything of a similar nature improvised in a country house at home.

Chloroform was administered by an Egyptian Medical Officer, who was stationed at Sollum, and I was assisted by the orderly whom I had brought with me from Cairo.

The abdomen was opened and found to contain foul-smelling pus. The appendix was gangrenous, and general peritonitis was present. The lower end of the cæcum showed a sloughing area about the size of a sixpence just external to the origin of the appendix. The pelvis also contained a quantity of pus. The appendix was removed in the usual way, and the sloughing area invaginated by a double layer of sutures. Great difficulty was experienced during the latter part of the operation owing to the friable condition of the cæcum, and matters were further complicated by the light failing twice whilst the cæcum was being sutured. Drainage was provided for by a suprapubic tube and by a tube through the original incision, after the pus had been gently mopped out of the abdominal cavity and pelvis. The patient stood the operation well. On recovery from the the anæsthetic he was placed in the Fowler position, and put on rectal salines four-hourly. On the morning of the 19th his condition was still critical, but as satisfactory as could be expected. The rectal salines were continued. On the 20th his condition was considerably improved. The tubes were draining well, and there was little or no distension. The pulse was good and there was no vomiting. A week's supply of sterile dressing was left with the Egyptian Medical Officer, and we left for Aboukir at 08.25 hours.

The return journey to Aboubir was uneventful, and we arrived at 11.45 hours. We had lunch at Aboukir, whilst some minor repairs were being carried out, and we left for Helouan at 15.10 hours, reaching there at 16.25 hours.

Subsequent reports from Sollum on the patient's condition stated that he continued to improve steadily. On the morning of the 22nd flatus was passed, and the bowels acted, and his condition was described as being very satisfactory. At about 18.00 hours on the same day, his breathing suddenly became embarrassed, and his face cyanosed, and he collapsed and died within a quarter of an hour.

No post-mortem examination was held, but it would appear that death was due to pulmonary embolism.

Echoes of the Past.

THE EXPEDITION TO THE SCHELDT, WALCHEREN, 1809.

Amongst some old papers in the War Office have been found a few personal notes on the Walcheren Expedition by Sir James McGrigor; the notes are in his own handwriting and relate to the conditions he found when he arrived in Walcheren in relief of Mr. Webb, who was ill with fever.

The English Government thought by striking a blow in the Scheldt to frustrate Napoleon's hopes of maritime rivalry at the outset and further to detain in Holland a portion of the forces the Emperor was at that time setting in motion against Austria.

To effect these objects an expedition was sent to the Scheldt, and on July 16, 1809, Lieutenant-General the Earl of Chatham, K.G., was

appointed to the command of land forces destined to attack and destroy Flushing in the island of Walcheren, Terneuse and Antwerp on the West Scheldt, and to render this river unnavigable for ships of war. Lieutenant-General Sir Eyre Coote was second in command, and according to a return in the Adjutant General's office, dated July 15, 1809, 40,143 troops were put under orders, but according to a further note dated February 1, 1810, it appears that 1738 officers and 37,481 serjeants, trumpeters, drummers and rank and file actually embarked for service. Mr. Webb, Inspector-General of Hospitals, was on the General's staff.

Rear-Admiral Sir Richard Strachan was appointed to the command of the fleet destined to convey the troops and to co-operate with Lord Chatham's forces in the destruction of the naval establishments in the Scheldt.

On July 30, the English troops advanced towards Middleburg in Walcheren, and during the day, the garrison having been withdrawn, terms of capitulation were arranged. On August 1 an advance was made against Flushing in order to invest the town. Early in the morning of August 14, the French demanded a suspension of hostilities, and during the day terms of capitulation were arranged. From the time of landing to the capitulation of Flushing the English casualties were killed 117, wounded 586, missing 44.

The French made preparations for the defence of Antwerp, and Napoleon ordered Bernadotte to assume chief command. He arrived on August 15, and found about 20,000 troops had been assembled. By August 24, Antwerp was secure from a *coup-de-main*. Lord Chatham had lost seventeen valuable days in Walcheren, and even when Flushing had fallen he made no attempt to make up for lost time. Operations had been going on in South Beveland, and on August 25 Lord Chatham arrived at Fort Bath on the West Scheldt, where the whole army except the garrison in Walcheren was assembled. There were now some 23,000 infantry and 2,000 cavalry available for further operations, but in the face of the numbers which the French had collected Lord Chatham did not dare risk an attack on Antwerp. He assembled a Council of War which decided against action. Lord Chatham then wrote a letter home giving as his reasons for abandoning the object of the expedition, the strength of the enemy, and the fact that the climate was telling on the health of the troops, of whom there were already 3,000 sick. Orders were accordingly given to strengthen the garrison of Walcheren, and embark the remainder of troops for England. The sickness since known as the Walcheren fever had made terrible ravages amongst the men left on the Island of Walcheren during the operations in Beveland.

Sir Eyre Coote, writing from Middleburg, asked for additional medical assistance and suggested that three or four large hospital ships should be fitted up and sent from England, as he stated that the sea air was found to do them good. The alarming sickness from which the army was

suffering created the greatest anxiety in England, and Lord Chatham was directed to forward a report from the chief medical officer on the nature of the disease. This was furnished by Inspector-General of Hospitals John Webb. The following extracts are taken from his report.

"The island being so flat and nearly level with the sea, is little better than a swamp; the ditches are filled with putrid vegetable and animal matter; the quantity of pure water very limited. The inhabitants are sickly and infirm." "The sickly season begins about the middle of August and continues till the frost stops the exhalations from the earth; the dry hot weather causing the greatest amount of sickness." "Nearly one-third of the population is attacked with fever every sickly season, in spite of the greatest attention to cleanliness both in buildings and in person." "The fever first showed itself amongst the troops in South Beveland, who had not the opposition of an enemy to keep their minds and bodies in healthy action. But on the fall of Flushing it broke out amongst the troops in Walcheren." "At first the disease appeared as a low fever, but subsequently took a form similar to jail fever. It spread with unexampled rapidity." "No remedy could be devised to check the ravages, though means might be taken to mitigate the severity of the attacks." "Men who have suffered from this fever have their constitutions so shattered that their physical power will for the future be materially diminished."

On September 9, 1809, Lord Chatham handed over the command to Sir Eyre Coote and sailed for England the following day. On September 10, there were 220 officers and 8,095 men unfit for duty out of a force of 16,000, and the Inspector-General of Hospitals, Mr. Webb, was laid up with the fever. Though the medical officers displayed the greatest zeal the condition of the force was deplorable. Medical assistance had been demanded from England but had not arrived. The hospitals established at Middleburg, Flushing, Veere, Arnemuiden, Zoute-lande, and Rammekens were crowded. At Middleburg there were not sufficient beds to allow of each patient having one to himself. Mr. Barrow, the Deputy Inspector of Hospitals, reported that the fever was degenerating into typhus, especially amongst the men who had lately returned from Corunna. On September 19, there were 224 officers and 9,627 men sick. On September 23, Sir Eyre Coote, who had taken over the command from Lord Chatham, wrote home that the 23rd Regiment had been so weakened by fever that he had ordered it home at once; the 6th and 81st Regiments were so sickly that they had been struck off duty, whilst the 77th and 84th were nearly as bad. The doctors in Walcheren could render but little assistance to those of the English Army owing to the difficulty the men found in understanding the Dutch doctors and to the inferior medical attainments of the medical men themselves. In the report on the expedition to Walcheren published by the Intelligence Branch of the Quartermaster-General's Department, Dr. MacGregor (McGrigor), Inspector-General of Hospitals, is stated to have arrived on September 30, 1809. In the personal notes, Dr. McGrigor

says he left Portsmouth on September 21, arrived at Deal on the 23rd, and Flushing on September 29. In his autobiography, Sir James McGrigor relates how he sailed in the "Venerable" flagship of Sir Henry Popham, which ran on a sandbank near Flushing; after a terrible night he and the exhausted crew of the ship were brought to shore by the boats of the fleet then in Flushing, and the "Venerable" was also brought into harbour next day under jury-masts. Dr. McGrigor, on landing, was soaked with sea water from working at the pumps on the "Venerable." He was looked after by Dr. Forbes, and next day proceeded to Middleburg, where Sir Eyre Coote had provided a splendid billet for him in the house of a director of the Dutch East India Company. Dr. McGrigor then proceeded to inspect the hospitals. There were 9,609 sick, and the stores both of apothecaries and purveyors were drained of many articles of the most essential description. There had been a great consumption of bark and there was little in store. On the authority of Sir Eyre Coote, 1,460 pounds were purchased by Mr. Robert Stewart, Purveyor to the Forces. Bark was also purchased from an American vessel, and this supply lasted until the stores arrived from England.

By order of the Surgeon-General, 2,410 sets of bedding were shipped for the expedition under the command of the Earl of Chatham, and on July 19, a requisition was sent to the Storekeeper-General for 1,000 palliasses and 3,000 sheets, but only 1,000 palliasses and 1,300 sheets were shipped, there being no more in store.

On September 30, 1909, there were serving in Walcheren 2 deputy inspectors, 1 physician, 12 staff surgeons, 12 regimental surgeons, 23 regimental assistant surgeons, 3 apothecaries, 25 hospital mates, 1 purveyor, 3 deputy purveyors, and 9 clerks.

At the outset of the expedition, fifty men of the 7th Royal Veteran Battalion had been detailed as hospital orderlies and assistants to the sick. On the great increase in sickness, Sir Eyre Coote applied for 300 more men to be sent for service in the hospitals; this application was refused and the General was instructed to get his assistants from the natives at the rate of ten men per regiment. These men were termed regimental pioneers and received pay of one florin per day.

On completing his inspection, Dr. McGrigor recommended to the Home authorities that for the conveyance of sick and convalescents to England the guns should be taken out of a number of war vessels and that they should be fitted with hammocks; the invalids embarked to be under the care of the naval surgeons of the vessels used as transports. Accordingly four line-of-battle ships were fitted up as recommended and ordered to Flushing to bring home invalids.

In the personal notes Dr. McGrigor states that in his first letter to the Surgeon-General he pressed for medical officers, orderlies and bedding. On October 7, 1,000 sick under the care of Staff-Surgeon Porter were dispatched to Harwich, and on October 11, 1,600 more sick were shipped to

England. The return sent to the Surgeon-General by Dr. McGrigor from Middleburg on October 12 showed the total sick as 9,614 and the deaths per week as 218. There were 4,959 cases of fever. The monthly return sent on October 22 stated that notwithstanding 4,000 sick had been sent to England since the arrival of Dr. McGrigor the number remaining was 6,425. It was stated that the 23rd Regiment being unable to march was conveyed in wagons from Middleburg to Flushing where it embarked for home. At Dr. McGrigor's request a barrack department was established, some 400 Dutch being employed. At this period the troops had no warm winter clothing and no cook-houses had been built for their use. On October 26 out of 54 medical officers only 23 were fit for duty, and the average number of sick per regiment was 400 men.

In his autobiography Sir James McGrigor relates that at length the Government came to the decision that one of the members of the Medical Board should proceed to Walcheren and report to His Majesty's Government. The Physician-General, Sir Lucas Pepys, was ordered to proceed, but declined to go as he said he was not acquainted with the diseases of the soldier in camp or in quarters. The other two members of the Board did not volunteer their services, but ordered Dr. Borland, Inspector-General of Hospitals, and Dr. Lempriere, Physician to the Forces, to go out. Sir Gilbert Blane, who had been in the Navy, volunteered his services. These three gentlemen went out as Commissioners. They remained a few days and recommended the removal of the remainder of the army to England.

On October 26, Sir Eyre Coote handed over the command to Lieutenant General Don, who wrote home urgently demanding transports for the removal of sick.

After some correspondence as to the possibility of holding Walcheren, orders were received to destroy the works and basins of Flushing preparatory to embarkation.

From November 7 to 20, 1809, preparations were made for destroying the works and docks of Flushing, and in completing the embarkation of the sick in transports which had now arrived from England. The enemy made no attempt to molest the departing expedition, and in the course of a few days the several divisions sailed for England where all arrived before the close of 1809. Dr. McGrigor embarked in the "Asia" hospital ship and reached Deal on the morning of Christmas Day.

The expedition, which effected so little, cost the country about £850,000, and the lives of 4,000 men, of whom only 166 fell in battle.

In the year 1811, Thomas Wright wrote a book entitled "History of the Walcheren Remittent," which he dedicated to Francis, Earl of Moira. Wright had served in Walcheren as a Temporary Physician to the Forces. He criticizes severely the behaviour of the medical men: "Some even refused to do the duty and if they could effect their retreat to Flushing did not scruple to take leave to retire. This accounts for the difficulties and want of support felt but too severely by the Temporary

Physicians in a duty which few would undertake, more avoided; . . those of merit had compassion only to stimulate them to exertion, for so lightly did they hold the pay assigned to them that they appeared desirous of quitting the attendance without it, and I believe few have received it." He then quaintly observes: "Nor can it ever happen otherwise unless the Inspectors shall be permitted to return gentlemen for pay in proportion to their merits: for the ardour of humanity is damped in such cases seeing that the worthless and even criminal are just as well recompensed as the intelligent and the deserving."

Wright states that the sick from Walcheren were brought to Harwich, where the Government "humanely ordered" the barrack to be converted into a hospital. The transports on arrival were visited by the physicians and the worst cases only were selected for detention at Harwich; cases suffering from fever, but able to travel, were sent to Ipswich, and convalescent cases to Colchester.

The barrack at Harwich was on a "cheerful hill" and the soil of good declivity allowing for good drainage "should the artificial drains and sewers be obstructed." The houses or huts were of wood and "disjunct with wide intervening ways, every apartment opening to the street without communication with any other and every room ventilated through the ceiling." Wright says that "even if contagion could not be obviated, yet the insulated state of each ward would prohibit the progress of it, and if I may judge of the future by the past, render it impossible: for in the autumn of 1809, it was the most free from contagion of any hospital containing so many and such forlorn cases on record in any country." He further states that they lost only one in six of the "miserable cases" left at Harwich, while Sir John Pringle at a later period lost one in five of the unselected sick of a comparatively healthy army, the cases being treated at Ipswich.

Wright describes the Walcheren fever as a "Remittent fever." He says there was a continued type of fever, also a quotidian, tertian and quartan. "Indolent enlargement of the spleen, an ague-cake, attended the Remittent of Walcheren from an early date." The Walcheren convalescents suffered from three, four, or even five relapses, of whom several, though not many, died in England.

Current Literature.

Specific Leprous Reactions and Abnormal Vaccinia induced in Lepers by Smallpox Vaccination. By Oswald D. Denny, Public Health Reports, U.S.A., 37.51. December 22, 1922. 3141.

The Effect of Vaccinia on Leprosy. By H. E. Hasseltine, Public Health Reports, 38.1, January 5, 1923. 1.—At the United States Marine Hospital, No. 66 (National Leprosarium Carville, La.), 118 lepers and 105 non-leprous attendants were recently vaccinated with the same virus and the same technique throughout. The vaccinia in the non-leprous attendants ran a normal course.

Vaccinia in lepers was accompanied by more severe local symptoms than in normal individuals. This abnormal susceptibility to vaccine virus occurred in all types and stages of leprosy. Following vaccination, acute specific leprous lesions occurred in a proportion of cases sufficiently large to establish a relationship of cause and effect. These acute leprous reactions developed not only near the site of vaccination, but were generally distributed over the entire body, manifesting themselves as nerve disturbances in the nerve type, as highly inflammatory macules and nodules in the skin type, and as nerve and skin lesions in the mixed type. The acute leprous reaction to vaccination was of short duration. No case was permanently aggravated, and some showed actual amelioration. A symbiotic relation existing between vaccine virus and the bacillus of leprosy offers the best explanation of the phenomena observed.

There is strong evidence that these lesions were provoked by vaccination and were not merely intercurrent attacks of leprous fever with its usual skin changes.

Very similar results were observed in 219 patients at the Kalihi Hospital, Hawaii. Hasseltine observes that these phenomena may explain some of the opinions held by the older Hawaiin laity, that in many cases vaccination was the cause of leprosy. During the days of arm-to-arm vaccination the possibility of the spread of leprosy by this procedure must be admitted. It is more probable, however, that the vaccination served to bring to light latent or unrecognized leprosy.

Duration of Passive Immunity. By A. T. Glenny and Barbara E. Hopkins, *Journal of Hygiene*, 22.2. November, 1922.—The course of elimination of passive immunity was experimentally observed in rabbits injected intravenously with diphtheria antitoxin obtained from a horse. This elimination consist of three phases :—

(*a*) An initial loss of fifty per cent occurring within the first twenty-four hours;

(*b*) A gradual constant percentage loss of approximately twenty-five per cent from day to day lasting six to seven days;

(*c*) A rapidly accelerated loss of fifty per cent or more per day after the seventh or eighth day.

In rabbits sensitized with small doses of horse serum before the intravenous injection of the diphtheria antitoxin the same three phases are seen but their duration is somewhat different:—

(*a*) The initial loss remains the same;

(*b*) The gradual constant percentage loss is of the same magnitude, but lasts only two or three days;

(*c*) The rapidly accelerated loss occurs after the third or fourth day, and over ninety per cent of the antitoxin present is lost within twenty-four hours.

The Production of Spasms of the Diaphragm in Animals with a Streptococcus from Epidemic Hiccough. By Edward C. Rosenow, *Journal of Infectious Diseases*, 32.1. January, 1923. Pp. 41 and 72.—From eight cases of epidemic hiccough a green producing Gram-positive non-encapsulated elongated diplococcus arranged singly and in short chains was isolated by injecting material from the respiratory passages into the brains of animals. The intra-tracheal and intra-cerebral injection of subcultures of this organism produced spasm of the diaphragm and of certain other muscles in animals. These results were not obtained with streptococci from similar sources in other diseases. The possibility of an accompanying filtrable virus was excluded by filtration experiments and by the successful reproduction of the disease after many rapidly-made subcultures of the different strains. The conclusion that epidemic hiccough is due to a streptococcus having peculiar neurotropic properties seems warranted. The facts that it belongs to the pneumococcus-streptococcus group of organisms normally present in the upper respiratory track of of man, that contagion is usually not demonstrable in this disease, and that the power of producing this condition disappears as new and distinctive invasive powers become manifest from successive animal passage and artificial cultivation suggest that the peculiar localizing power of the hiccough strain is an acquired property, a phase perhaps in the life-cycle of the pneumococcus-streptococcus group of organisms.

Further experiments with living cultures, filtrates and the dead organism appear to indicate that the symptoms are due to the production of a specific toxin.

A New Medium for the Isolation of Bacillus Diphtheriæ. By S. R. Douglas, *British Journal of Experimental Pathology*, vol. iii, No. 6. December, 1922. P. 263.

The Selective Culture medium for the Diphtheria Bacillus. By E. A. Greenspon, *Bulletin of the Johns Hopkins Hospital*, vol. xxxiv, No. 383.

January, 1923. P. 30. Douglas' medium consists of agar and serum of which the anti-tryptic action is neutralized, with the addition of 0·04 per cent of potassium tellurite. It is stated that with this medium the diphtheria bacillus and closely allied organisms grow more luxuriantly, while other common throat organisms are inhibited. Comparison with Loeffler's medium showed that although the number of instances in which *B. diphtheriæ* was microscopically recognized was not markedly increased, the subsequent isolation of the organism was rendered much more easy.

Greenspon's medium consists of a Loeffler's medium with the reaction adjusted to pH 6·4 by means of three per cent citric acid solution. The morphology of the diphtheria bacillus grown on this medium does not differ materially from that on Loeffler's serum, but the growth is somewhat more luxuriant, and although closely allied organisms such as *Bacillus Xerosis* and *B. Hoffmanni* are not affected, it is stated that other throat organisms are almost entirely suppressed.

The Value of the Complement Fixation Reaction in Gonococcal Infections. By Allen S. Walker, *Medical Journal of Australia*, vol. II, 9.23. December 2, 1922.—The serum reactions of a series of over 500 patients, mostly male, were investigated. A general account is given of the inquiry and concordance is established with the other published work on the subject of which an extensive bibliography is given. There is also a description of the technique, stress being laid on the need for a sensitive method.

The author concludes that the value of the test is considerable; that it confirms the clinical findings, and directs attention to those cases worthy of more intensive study or treatment; that it sheds light on cases difficult of diagnosis, and helps in forming an estimate of the prognosis, constituting another and reliable link in the chain of evidence of cure. In the light of the experiences gained during this research he states that he would not feel justified in sanctioning marriage in any patient whose serum fixed complement in the test.

The Use of Anti-Gonococcal Serum in the Treatment of the Complications of Gonorrhœa. By Allen S. Walker, *Medical Journal of Australia*, vol. II, 9.26, p. 731. December 23, 1922.—Sixty-four patients in various stages of the disease and with various complications, were treated with a polyvalent serum prepared by immunizing a horse with five strains of organisms freshly isolated from active infections. The serum was given in doses ranging up to at least thirty cubic centimetres, and improvement, when it occurred, was found after two or three injections by the intramuscular or subcutaneous route. The use of the serum was attended by more definite, rapid, and more lasting success in patients whose infecting organism was homologous to the strains used in preparing the serum as judged by the findings of complement fixation tests on their sera. It was found of distinct benefit in most of the acute complications of gonorrhœa,

especially arthritis and prostatitis, but, while it may ameliorate, it does not appear to cure the original lesion.

Experimental Studies of the Nasopharyngeal Secretions from Influenza Patients. By Peter K. Olitski and Frederick L. Gates. Authors' summaries of a series of papers published in the *Journal of Experimental Medicine.* Memorandum circulated by the Ministry of Health.

(I) Transmission Experiments with Nasopharyngeal Washings. (February 1921.)

An active substance has been detected, by the methods described, in five patients in early stages of epidemic influenza during 1918-19 and two patients in early stages of epidemic influenza during 1920. It was not detected in twelve cases of the same disease in which the onset of obvious symptoms occurred more than thirty-six hours before washing of the nasopharynx was carried out, nor was it found in the secretions of fourteen individuals free from the syndrome of influenza either during the epidemic or the interval between them.

With this substance a clinical and pathological condition has been induced in rabbits affecting the blood and pulmonary structures mainly, which could be maintained and carried through at least fifteen successive animals. For this reason, and also because of the dilution between passages, we are led to believe that we were dealing with the actual transmission of a multiplying agent rather than with a passive transference of an original active substance.

In some of the experiments secondary infections by ordinary bacteria were encountered. The relation of these micro-organisms to this active substance will be dealt with fully in another communication. However, the essential effects were produced by a substance wholly unrelated to those bacteria.

The similarity that exists between the effects produced in rabbits on the blood and the lungs and those occurring in man in epidemic influenza provided a basis for further investigation on the inciting agent of epidemic influenza.

(II) Filtrability and Resistance to Glycerol. (March 1921.)

The experiments here reported indicate that this active agent has the following properties.

(1) The agent as it exists in the nasopharyngeal secretions in man, and in the lungs of rabbits injected with the human secretions, passes through Berkefeld V and N candles.

(2) The filtered material produces the same effects on the circulating blood and on the lungs of rabbits as the unfiltered material.

(3) The peculiar effects described as arising in the inoculated rabbit may also be induced in guinea-pigs inoculated with the agent.

(4) The agent responsible for the reaction on the blood and the lungs of rabbits withstands the action of glycerol in a sterile fifty per cent solution, for periods up to nine months. The question must be left open at present whether the agent can withstand longer contact with the chemical. In two experiments after ten and a half months' contact the agent induced no observable changes in the blood and lungs of rabbits.

(III) Studies of the Concurrent Infection. (March 1921.)

(1) Concurrent infections in the experiments described may be regarded as of accidental nature and are not causally related to the typical effects induced in rabbits by a material wholly free from ordinary bacteria.

(2) The influenzal agent exerts an effect on the pulmonary tissue which encourages the invasion of the lung and subsequent multiplication there of ordinary bacteria, such as the pneumococcus, streptococcus and *Bacillus pfeifferi*.

(3) A similarity is believed to exist between the conditions under which concurrent infections arose in the inoculated rabbits and those which seem to favour the occurrence of concurrent infections during epidemic influenza in man. In no instance did death occur in the rabbits as a result of the uncomplicated effects of the influenzal agent alone. When death occurred in any of the inoculated animals concurrent infection of the lungs by ordinary bacteria was present. The micro-organisms most commonly met with under these conditions were pneumococcus Type IV and a typical Type II streptococcus, and hæmoglobinophilic bacilli. Other kinds were encountered less often.

(IV) Anaerobic Cultivation. (June, 1921.)

From the filtered nasopharyngeal washings of patients in the first thirty-six hours of uncomplicated epidemic influenza and rarely in later stages of the disease, we have cultivated a minute bacilloid body, *Bacterium pneumosintes*, 0·15 to 0·3 micron in length, of constant cultural characters and capable of indefinite propagation on artificial media. This organism, not of the nature of ordinary bacteria, was also recovered in pure culture from the unfiltered and filtered lung tissue of rabbits and guinea-pigs inoculated with unfiltered and filtered nasopharyngeal washings of early influenza cases, both from the first epidemic of 1918-19 and from the second one of 1920. The organism grows only under strictly anaerobic conditions, passes Berkefeld V and N filters, and withstands the action of sterile fifty per cent glycerol for a period of months.

It has been recovered from cultures contaminated with a variety of ordinary bacteria such as *B. pfeifferi*, pneumococci, streptococci, and staphylococci, and has been experimentally cultivated in symbiosis with them.

Similar cultivation of control materials uniformly failed to yield growths of this organism. The materials tested consisted of the unfiltered and

filtered nasopharyngeal washings of persons free from influenza, some of whom were suffering from acute coryza, the lung tissue of normal rabbits and of rabbits with bacterial respiratory infections, and the uninoculated media.

The intratracheal injection in rabbits and guinea-pigs of mass cultures of this organism has induced effects on the blood and lungs of these animals which are not to be distinguished from those obtained with the naso-pharyngeal secretions of patients in the early hours of epidemic influenza. From the pulmonary lesions thus induced the same organism has been recovered in pure culture, and has been found to cause similar lesions on subsequent animal passage. Its pathogenicity is not lost by prolonged artificial cultivation.

Our experiments indicate that the cultivable bodies obtained directly from human nasopharyngeal washings and from affected rabbit lungs are strains of the same organism. This organism appears to be the source of the reactions which occur in experimental animals—rabbits and guinea-pigs—as a result of the intratracheal injection of nasopharyngeal washings obtained during the early hours of uncomplicated epidemic influenza in man.

(V) *Bacterium pneumosintes* AND CONCURRENT INFECTIONS. (July, 1921.)

During the course of animal experiments with the anaerobic filter-passing organisms cultivated from epidemic influenzal sources, certain pulmonary infections with ordinary bacteria have been observed. The experiments also have shown that the lungs of animals infected with *Bacterium pneumosintes* are less resistant than normal lungs to infection with ordinary bacteria. The demonstration of this fact invites a comparison of the course of these experimental bacterial infections with the sequence of post-influenzal pneumonias attributable to similar organisms in man.

These observations furnish additional proof of the identity of *B. pneumosintes* and the active agent derived from the nasopharyngeal secretions of patients in the early hours of epidemic influenza.

(VI) IMMUNITY REACTIONS. (January, 1922.)

The experiments described furnish additional evidence of the pathogenic character and the virtual identity of the various strains of the active agent derived from the nasopharyngeal secretions of influenzal patients with which the transmission experiments in rabbits have been carried out.

The active material has been shown to be of antigenic nature, so that rabbits are protected from the effects of a second inoculation. The experiments indicate also the antigenic identity of the various strains of the active agent with each other and with *B. pneumosintes*.

Finally, the experiments show that the protection may persist for fourteen months which is the longest period yet tested.

(VII) SEROLOGICAL REACTIONS. (April, 1922.)

Cultivation of *B. pneumosintes* in the collodion sac dialysate of a tissue medium produces an antigen suitable for serological tests.

Injection of dialysate cultures of *B. pneumosintes* into rabbits results in the production of antibodies demonstrable by agglutination, precipitation, complement fixation, and phagocytic reactions.

Four strains of *B. pneumosintes*, three from the first epidemic influenzal wave (1918-19) and one from the second (1920), show identical antigenic characters.

The blood serum of rabbits experimentally injected with the glycerolated active material of rabbit passages contains specific agglutinins for *B. pneumosintes*, whereas normal rabbit serum does not.

(VIII) FURTHER OBSERVATIONS ON THE CULTURAL AND MORPHOLOGICAL CHARACTERS OF *B. pneumosintes*. (June, 1922)

After artificial cultivation for a period of over three years *B. pneumosintes* has maintained its original morphological and cultural characteristics, when grown in the original medium. Adaptation to a saprophytic existence has been accompanied by a loss of pathogenicity. Our strains now grow readily under strictly anaerobic conditions in a variety of media with peptone broth as a base, enriched with fresh tissue, blood, or by growth of other bacteria. Surface colonies have been obtained on blood agar plates in an anaerobic jar. These various methods of cultivation are adapted to special purposes. In broth cultures *B. pneumosintes* grows in larger forms than in the ascitic fluid-tissue medium, but the identity of the micro-organisms is proved by their serological reactions and by reversion to the minute forms on transfer to the original medium.

(IX) THE RECURRENCE OF 1922. (November 1922.)

From the nasopharyngeal secretions of patients in the early hours of uncomplicated epidemic influenza during the recurrence in New York City in January and February, 1922, we have again obtained an active agent, pathogenic for rabbits, and have identified this active agent as *Bacterium pneumosintes*. Four new strains of this micro-organism have been isolated in pure culture and identified with the 1918-19 and 1920 strains on morphological, cultural and serological grounds. All of the significant characteristics of the old strains, including their effect upon the resistance of the lungs of rabbits to secondary invasion with other bacteria, have been noted in the new strains, which thus have served to confirm and extend our original observations.

(X) THE IMMUNIZING EFFECTS IN RABBITS OF SUBCUTANEOUS INJECTIONS OF KILLED CULTURES OF *Bacterium Pneumosintes*. (December 1922.)

A series of rabbits was subcutaneously injected with three measured doses of killed cultures of two strains of *B. pneumosintes* derived from the

nasopharyngeal secretions of influenza patients. These rabbits were subsequently tested for the development of serum antibodies and for the presence of an induced immunity to the living organisms, with the following results.

The serum of eleven rabbits, tested from ten to twenty-seven days after the final subcutaneous injection, specifically agglutinated *B. pneumosintes*, whereas normal rabbit serum did not.

Nineteen vaccinated rabbits were subjected to protection experiments. Two of them were unaffected by an intratracheal injection of *B. pneumosintes*, contained in the lung tissues of previously infected animals, in a dose which typically affected the control rabbits. Fifteen of the other seventeen proved to be completely resistant when tested by intratracheal injections of *B. pneumosintes* cultures that produced typical infections in the controls. Ten of these fifteen rabbits were injected intravenously with living cultures of pneumococcus, *S. hæmolyticus*, or *B. pfeifferi* in doses which were non-infective under normal conditions, but infective, as experience has shown, in the presence of a primary lesion caused by *B. pneumosintes*. In none of these animals did infection develop. The two remaining rabbits of the seventeen were not protected against *B. pneumosintes* by the vaccination, and they further developed a secondary pulmonary infection with *B. pfeifferi* after its intravenous injection. Control rabbits similarly injected intratracheally with *B. pneumosintes*, and then intravenously with the pneumococcus, streptococcus, or *B. pfeifferi* in doses that had proved non-effective for normal rabbits, uniformly developed a secondary infection with these organisms.

The mildness of the local reactions and the absence of general signs, following vaccination with *B. pneumosintes*, indicate that similar injections would be well tolerated in man. There is no evidence that the subcutaneous injection of large doses of the heat-killed organisms reduces the resistance of the animal body to infections with other bacteria. In single rabbit experiments the resistance to intravenously injected pneumococci, streptococci, or *B. pfeifferi* has been found unreduced immediately after vaccination with *B. pneumosintes*.

Reviews.

SECOND NOTICE.

SURGERY OF THE WAR. Vol. I. Edited by Major-General Sir W. G. Macpherson, K.C.M.G., C.B., LL.D., Major-General Sir A. A. Bowlby, K.C.B., K.C.M.G., K.C.V.O., Major-General Sir Cuthbert Wallace, K.C.M.G., C.B., and Colonel Sir Crisp English, K.C.M.G. Pp. 618, with 16 coloured plates and numerous illustrations. Printed and published by His Majesty's Stationery Office, 1922. Price £1 5s.

In Chapter XII Sir G. Makins sums up the battle of the antiseptics and gives a fair and impartial account of all the various methods by which surgeons attempted to sterilize a wound.

We note with pleasure the prominent place given to the work by Sir Almroth Wright. From the earliest days of the war Wright insisted on efficient surgery as an indispensable part of any attempt to sterilize a wound, and subsequent developments showed that this was correct.

Although the importance of primary suture, delayed primary suture and early secondary suture are emphasized as the objects to be aimed at; the difficulties of complete ablation of a wound and the continued necessity for antiseptics are pointed out.

We can strongly endorse the advice not to rush a badly wounded patient into a second operation directly he arrives at a base hospital because of some elevation of temperature and unfavourable signs in the wound. One or two days rest and careful local treatment will often render an operation unnecessary.

Some of the methods are given in greater detail than others, for instance, the Carrel-Dakin method is only broadly described, and there are no details of the method of operation or application of the solution to the wound. The method described for preparing the Dakin-Daufresne solution is also incomplete.

The records of the observations carried out in several series of cases, principally at the Etaples base, are instructive, and lead us to the conclusion that none of the antiseptics described or methods adopted could be expected to give satisfactory results without careful personal attention from the surgeon in charge of the case. If this condition was fulfilled any or all of the various methods advocated were satisfactory.

The way in which thorough surgery of the wound at the earliest possible moment influenced the surgical work at a base is well brought out in the article.

This chapter differs very much from the description of the Casualty Clearing Station. No mention is made of the machinery for dealing with surgical patients in base hospitals. This is rather a pity, for although the changes were not so dramatic as those effected in a casualty clearing station, yet as originally equipped and staffed, a general hospital was very ill-designed to cope with sixty or seventy operations in twenty-four hours as frequently occurred.

One of the great difficulties met with was the meagre information received as to the surgical procedures carried out at the casualty clearing station, and another the transmitting to the home hospital some record of the progress of the case and further operations performed at the base. How these difficulties were in part overcome is worth recording.

The classification of cases at the base was also a further factor in the successful treatment of the wounded. The special femur hospitals, jaw treatment and eye sections were a most important step in securing for the wounded the most skilful treatment for his particular injury by officers with special experience.

No doubt much of this was considered unnecessary in this section of the book, as it will probably be dealt with when the injuries of special regions come to be considered, but some general summary of the methods adopted at the base would be an improvement, and would make the volume more useful as a book of reference for administrative officers when considering the necessary arrangements for base medical units.

Confined as it is to general wound treatment, and excluding special infections like gas gangrene and tetanus, it is an excellent account, and will be read with pleasure and profit by all surgeons, but it gives only a very limited view of the general activities of a base hospital in France.

Chapter XIII is devoted to the surgical work in Palestine, Mesopotamia and Macedonia.

Colonel H. Wade writes on the surgical work in Palestine. The article is too short, and the amount of actual wound surgery is too small to enable us to draw much in the way of conclusions.

The outstanding features are the reversion to the type of wound we saw in

the South African War which is attributed by the author to the nature of the soil of the country over which hostilities took place. This is rather at variance with Sir G. Makins's contention, that perhaps too much stress is laid on the soil as a factor in wound infection.

The military operations being carried out mainly by cavalry, brings out prominently the necessity of a field ambulance acting at times as an operating unit.

Casualty clearing stations are hardly mentioned, and the mobile operating sections, which proved most useful, were worked in conjunction with field ambulances.

No mention is made of the retention of patients after serious operations, and evidently lady nurses were not employed in front line units.

Wounds of the chest appear to have again become largely the province of the physician, and only a few were operated on.

It is interesting to note that gas gangrene did occur even in conditions approximating to the South African War.

With regard to antiseptics, only two were relied on, namely, eusol and B.I.P.

The account of the surgical work in Mesopotamia, compressed into eight pages of text, gives us little real insight into the conditions that prevailed there. No new work is brought to light, and any improvements that were effected filtered through from the Western front late in the war.

It does, however, serve to illustrate again the fact that the conditions which prevailed in France, where casualty clearing stations remained fixed for months or years within a few miles of the firing line, may never again occur.

It would be interesting to know if Thomas's splints ever reached the front line units in Mesopotamia, and a description of a case of fractured femur evacuated on a springless Indian ammunition cart would have been instructive.

The times occupied by the journey of a wounded man from the front line to an operating centre are not given, but it apparently was such that abdominal cases had no chance. Is it not a fact that two or three days were occupied in the journey to a casualty clearing station sixty miles away in the concluding battle of the campaign?

The remarks on shock as seen in that country are interesting.

The whole of the surgical work in Macedonia occupies only two pages of text, and seems to show that the work was similar to that on the Western front, but that there was rather more difficulty in transporting cases to the operating unit.

It is interesting to note that of twelve abdominal cases operated on at the base, nine recovered; this recovery rate is out of all proportion to the experiences at the base in France, where late operations were very fatal. A detail of the lesions found in these twelve cases would be instructive.

Another remarkable achievement which is recorded is that where Carrel's treatment of a compound fracture of the femur was started at the casualty clearing station not a single limb was lost.

Here we would be interested to have the details of how the treatment was carried out in the journey from front to base. Carrel trains in France and boats to England were a conspicuous failure.

The incidence of 3·98 per cent of gas infection in all wounds is high, and makes the rarity of tetanus all the more remarkable. It is probably only a further corroboration of the value of tetanus prophylaxis.

It is quite impossible that anything like a complete account of the war surgery in this theatre of war could be given in this limited space.

We admire the skill of the authors in giving so much information in so few lines, but as a surgical history of the Great War in Macedonia it cannot be considered as adequate.

Colonel C. J. Bond, in a thoughtful and well-considered article on wound treatment in hospitals in the United Kingdom, makes no secret of the fact that this was unsatisfactory until the very end of the war.

The difficulties of dealing with large septic wounds in their later stages, and the reasons for these difficulties, are indicated.

The failure of the Carrel-Dakin method as applied to wounds on a long oversea and rail journey is strongly emphasized, and the great advantage of complete excision and early suture of the wound insisted on. Further, his plea for earlier secondary suture is sound. He justly points out that a wound which was completely operated on but not closed, did better than a failed primary suture. This is in favour of the ruling that when primary suture is carried out the case should be retained for at least seven days under the care of the surgeon who performed the suture.

The great fear among surgeons at home of stirring up "latent" infection is given as one of the reasons for non-interference in many cases which would have been benefited by operation. He shows how this danger could be minimized by preliminary treatment of the wound.

He gives an interesting explanation of the possible method by which depot antiseptics like B.I.P. act.

His criticism of the surgical personnel of the home hospitals is probably fully justified. The general practitioner had not the knowledge and experience necessary to deal surgically with serious war wounds. The author states:—

"A few of the medical officers gained this very necessary skill and knowledge quickly, a considerable number more slowly, and a certain number never gained it at all."

We have seen this emphasized in France, when in anticipation of heavy casualties a dozen or more freshly imported officers were attached for duty to a big surgical hospital. These officers were quite useless for the work, they could not, and wisely would not operate. They knew nothing of the routine methods employed for wound treatment in the wards, and had never before dealt with a compound fracture of the femur. They usually acted as spectators while the small permanent staff of the hospital carried on. In justice to these practitioners it must however be recorded that they compared very favourably with the general practitioners of any of our allies.

Colonel Bond also points out that where an officer had surgical skill and experience he was often not properly utilized, but shared the general routine work in the wards.

It was a long time before the importance of pathological, X-ray and electrical departments was recognized and the departments were utilized. There were no splint mechanics, and the lack of orthopædic knowledge allowed deformities to arise in the early stages which required prolonged treatment to rectify.

In many hospitals each officer worked on his own, and there was no co-operation and no team work. We have seen such a state of affairs exist in France, and at once rectified by collecting all the officers out of hotels and lodgings and bringing them together into a mess. Not only is co-operation necessary, but the "unit spirit" is essential to the successful working of any war hospital.

The lack of classification of cases was also a barrier to efficient treatment, and the conditions of the wounded were much improved when this was carried out.

We note that it was late in the war before the knowledge of the methods of treating fractures of the femur, gained in France, was introduced to home hospitals.

The article concludes with a plea for training in military surgery during peace. It is doubtful if the instruction of the general practitioner in civil hospitals will succeed in this object.

At present only regular officers of the R.A.M.C. are instructed in the principles of military surgery, and on the outbreak of a serious war it is unlikely that many of them will be available for actual surgical work, although it will help them in making efficient arrangements for the treatment of the wounded.

This is the most straightforward account given in the volume of early failure and the reasons for it.

We rather hoped to find some account of how certain types of wounds progressed; the average time taken for fractured femurs to unite; the number of amputations required for this condition; what alteration took place after the order was issued to retain these cases in France until some union had taken place; the sequalæ of head cases, etc., etc.

Possibly these were considered outside the scope of the article and may be dealt with under the special regions. It is this knowledge that would be so valuable in arriving at conclusions as to the best line of treatment at the front and without which no further progress will be made.

This clear statement of the work in home hospitals should be closely studied by every administrative medical officer so that the mistakes made in the late war may be avoided in the future.

Having noted that the article on wounds of the chest is contributed by Colonel T. R. Elliott and Colonel E. G. Gask, we look for an authoritative statement on the subject, and are not disappointed.

Not only is it an excellent example of collaboration in the writing of a scientific article, but illustrates the importance of the association of physician and surgeon in the treatment of gunshot wounds of the chest.

In this particular branch of war surgery it was the physicians who first pointed out the importance of operation in certain cases, and, but for their work, the progress of chest surgery would have been much delayed.

Colonel T. R. Elliott took a leading part in this work, and consequently what he writes carries great weight.

Colonel Gask was one of the first who followed the suggestions of Pierre Duval in what might be termed the complete surgery of the chest.

The article is complete in almost every detail, not only historically but also from the modern medical and surgical standpoint.

Many interesting statistics are given which of necessity only deal with the cases that survive to reach the medical services, and consequently we are still left guessing as to the total mortality of chest wounds, but there is no doubt that it reached a very high figure.

Those who have not before studied the subject will be surprised at the high mortality associated with sepsis in the pleural cavity. The steady fall in this figure as surgical methods were perfected is worthy of notice.

The surgical progress is traced from our first timid aspiration of a hæmothorax through the stages of imperfect drainage up to the final complete opening of the chest, clearing of all clot and foreign bodies and final closure of the wound.

The case for this latter operation is fairly stated, and the type of case in which it should only be employed clearly indicated. As thirty per cent of these cases subsequently developed an empyema, with the average skill among military surgeons, although the figure was much lower in the hands of a few experts, the authors are justified in not going quite so far as Pierre Duval in the advocacy of this method.

From the physician's point of view the physical signs in the chest are very complete and interesting. The early detection of the formation of gas by the anaerobic gas-producing organisms did much to save life. Massive collapse and the interesting condition of contralateral collapse of the lung are well worked out, and the importance is shown of deferring operation until this condition has cleared up.

The pathology and bacteriology of chest wounds, much of which is due to the work of Elliott and Henry at the Boulogne Base, is complete and convincing.

The operative methods are given with minute details, and the whole article will serve not only as history, but as a complete monograph on the subject.

The Carrel Dakin method after thoracotomy where closure is not carried out is

shown to be valuable in shortening the convalescence of the patient and assisting expansion of the lung. Many cases can be closed by secondary suture.

Throughout the article the value of the exploring needle as a means of diagnosis is strongly urged.

The final history of operated cases is traced and the measures recommended for hastening their convalescence are described.

For persistent empyema the value of decortication of the lung as opposed to Estlander's method is discussed, the authors advocating the former.

We should like to see some figures in support of this contention, as in our experience decortication in any form is a very serious operation, whereas, in spite of its mutilating effect, Estlander's method often affects a cure with less risk to the patient.

The figures given of the disability of pensioners, based on their assessment by Boards, are very important and correct the erroneous impressions we are apt to form of the seriousness of the after-effect of chest wounds.

It is admitted that the type of surgery carried out on the chest at the end of the war was only possible under the special conditions then existing, i.e., early arrival of the wounded at the operating centre, skilled surgeons experienced in this class of work, retention of the case after operation, and skilled nursing. The necessity for X-ray examination is also shown.

This is only a slight sketch of this excellent article, which must be read to be appreciated. It is well illustrated by radiograms, drawings, and two coloured plates.

In Chapter XVI Sir G. Makins deals with the subject of wounds of the pericardium and heart.

In this section of the book a different method is introduced to discuss the subject compared, with previous chapters.

The recorded cases of wounds of the pericardium and heart which survived to reach the surgeon were very few indeed, and Sir G. Makins has made the very most of the material at his disposal, and for subject matter draws largely on the excellent specimens which are preserved in the War Office collection in the museum of the Royal College of Surgeons.

The clinical history and pathological appearances of these cases are described in detail, and important and interesting deductions made.

This article is by far the best illustrated in the whole volume, and suggests that other sections of the book could have been improved in this respect.

The various and puzzling clinical manifestations of injury to the heart and pericardium are fully described and carefully analysed.

The methods of surgical approach to the heart and the technique to be employed in removing missiles and dealing with wounds of the organ are clearly given in detail.

The number of cases quoted is perhaps too few to justify dogmatic conclusions, but even in the few submitted to surgical operation a recovery rate of 50 per cent, which is almost on a par with our results in abdominal cases, should be an encouragement to surgeons in dealing with this very difficult branch of surgery.

At first sight it might appear open to criticism to devote forty-four pages to the discussion of a subject which after all could have a small bearing on the outcome of the war, as compared with sixty-one pages devoted to the treatment of wounds in general hospitals. On the other hand the very rarity of these cases enhances the importance of a complete record being preserved in the Surgical History of the War, as showing the possibility of satisfactory intervention in an otherwise hopeless class of case.

Wounds of the abdominal viscera are dealt with in Chapter XVII by Sir Cuthbert Wallace, and a small section is contributed by Colonel A. Fullerton on wounds of the urinary organs.

With slight modifications the article follows closely Sir C. Wallace's "War Surgery of the Abdomen," published in 1918. As this has previously been reviewed, no lengthy description is necessary. It is without doubt the most valuable contribution that has been made to our knowledge of the surgery of the gunshot wounds of the abdomen.

The article is an actual analysis of a definite series of cases, the principal deductions being drawn from a series of 965 cases operated on at the casualty clearing stations by various surgeons, working under a variety of conditions, so that the personal factor is largely eliminated, and a good idea can be gained of the average results we may hope to attain.

The various reasons for non-operative treatment of abdominal wounds at the beginning of the war are mentioned, and then the steady growth of the operative period is described.

That the casualty clearing station developed into the main operative centre in the fighting zone was undoubtedly due to the demand for early surgical treatment of abdominal cases, although it was realized later that all wounds required this early surgery.

The importance of the time factor in abdominal cases is well brought out and goes far to explain the lack of success in these cases in previous wars.

It was a series of post-mortem examinations which first showed that hæmorrhage was the main cause of death in abdominal wounds and consequently unless this was promptly dealt with surgically, the hope of recovery was very small.

If the table showing the percentage of recoveries in the various years after operation became general is examined, it is at first surprising to see the apparently slight improvement that occurred. This is easily explained by the fact that in the late years, with improved transport, far more of the serious cases reached the surgeon, and further the conception as to what should be regarded as a moribund case had undergone revision, so that increasing numbers of serious cases were submitted to operation.

The technique of operations on the abdomen for gunshot wounds is fully described and in dealing with the intestine the question of suture of the lesion as opposed to resection is discussed. It is interesting to note that the pendulum was swinging back to end to end anastomosis of the bowel instead of closure with lateral union of the gut.

As compared with the pre-operative days it is clearly shown that a large proportion of lives were saved by the introduction of routine operation.

The only type of injury in which the operative figures show a higher mortality than when left alone is colon wounds.

Colonel Fullerton's contribution on wounds of the urinary tract is a valuable addition. He discusses the conditions as seen at base hospitals. He throws much light on the question of interference with renal function after wounding, and excellent coloured plates illustrate the pathological changes in the kidney and the cystoscopic appearances of bladder wounds. The policy of conservative surgery for uncomplicated urinary fistulæ is advocated.

The whole article is well illustrated and the statistical tables will repay close study.

A statement such as this, showing the results of abdominal surgery in war, is required to justify such a radical change in our views of the treatment of war injuries of the abdomen. It has been said, "was all this abdominal surgery worth while?"

Remembering that abdominal wounds constitute only about two per cent of the total wounded reaching an operating centre and that one in five of them is moribund, and further that abdominal operations are very time-consuming, it has been argued that if the time was devoted to dealing early with more of the ordinary wounds of limbs, a greater economy of man power would result.

To judge of this it is necessary to take into consideration not only lessened mortality from operation, but also the question of final fitness for service. It will be seen from these tables that the number of men returned to duty is not inconsiderable, and would in itself justify operation, but the far more important question is the effect on the morale of the troops. To know that without operation the mortality was in the region of eighty per cent, and that after operation the mortality fell to about fifty per cent shows such a remarkable saving of lives, that we should never engage in war again without some arrangements for abdominal surgery near the line.

All these figures are from the Western front with a stable fighting line and casualty clearing stations so situated that cases could arrive in from six to ten hours after being wounded. It is well known that in other theatres of war these conditions did not exist, but we are left in ignorance of the result of abdominal wounds in these areas. The conditions experienced in France may never recur again, but this able article is a successful plea for some arrangement near the fighting line where abdominal operations can be performed and cases retained and nursed after operation.

Sir William Macpherson's preface begins with the following sentence: "The unparalleled number of battle casualties which occurred during the war gave the consulting and specialist surgeons who were employed with the British medical units an unrivalled opportunity for surgical work. . . ."

Does this volume, with its companion volume to follow, give a complete account of the surgery of the war?

The volume is somewhat inconsistent in that it is designed as history, but in parts becomes a textbook on military surgery, and this latter part is much more convincing than the former.

From the historical aspect it would have been improved if the early difficulties and failures had been more completely described, for by doing so the advances made would have been more strongly emphasized.

The operations on the Western front necessarily dominate the work, but very erroneous conclusions may be drawn unless the surgical work in other areas is fully described. The conditions which existed in France with a fixed line of battle and a short line of communication permitted of surgical arrangements which were quite impossible elsewhere.

Very little is known about these other theatres of war, as descriptions already published are very meagre. It was hoped that this surgical history of the war would give a complete account of the surgical work on all the fighting areas.

In this respect the reader will be seriously disappointed. The accounts of Mesopotamia, Macedonia and Palestine are much too brief to be of real value, although as far as they go they are interesting, but why have other battle areas been omitted completely? Surely there were special features regarding the wounded in Gallipoli which were worth recording?

Were there any wounded in German East Africa or German West Africa?

The reader will want to know something of the equipment and personnel of medical units in Mesopotamia, where these units were situated in regard to the fighting line, and the means and rate of transport of the wounded to a place where operations could be performed. Further, some information as to the mortality in abdominal and head cases in these other areas might correct our views as to the average results to be expected in war.

The description of surgery in home hospitals is an honest article, but it is too general and lacking in detail to prevent the same mistakes occurring in a future war.

With these reservations the book is a good one. The various articles are well written by the authors, and undoubtedly the best are those devoted to wounds of special regions, so that if the same excellence is maintained in the second volume, the whole should constitute a valuable surgical record.

The book has been well edited, and the proof reading well done. Only one error was detected,—on p. 545, where the term "common cystic duct" is used.

Some of the articles are very well illustrated, but others could be improved in this respect considering the unique pathological specimens and drawings preserved in the museum of the Royal College of Surgeons.

The volume is supplied with a good index.

TSETSE FLIES, THEIR CHARACTERISTICS, DISTRIBUTION AND BIONOMICS WITH SOME ACCOUNT OF POSSIBLE METHODS OF THEIR CONTROL. By Major E. E. Austen, D.S.O., and Emile Hegh. Pp. 188. Price 7s. 6d.

This monograph deals with the nineteen known species of *Glossina* besides several additional races or varieties, all of these being confined to Africa with the exception of one species which is also found in Arabia. For the descriptions of these flies the authors draw largely on Austen's "Handbook" (1911), the following additional species recognized since the appearance of that work being included: *G. austeni* Newst.; *G. ziemanni* Grunberg; *G. haningtoni* Newst. and Evans; *G. severeni* Newst.; *G. schwetzi* Newst. and Evans. *Fuscipes*, *maculata*, and *submorsitans* are not regarded as valid species.

The most important species of tsetse fly are *G. palpalis* and *G. morsitans*, which are ordinarily the invertebrate hosts of *Trypanosoma gambiense* and *T. rhodesiense* respectively; the latter organisms can develop in *G. brevipalpis* also, and it is likely that many species of *Glossina* may be efficient hosts for trypanosomes.

As is well known, tsetse flies do not lay eggs, the female producing a single mature larva at a birth; on extrusion the larva conceals itself usually by burrowing into the soil where it pupates. Very interesting accounts of these various stages are given, which include differential descriptions of the larvæ and pupæ of several species.

About thirty pages are devoted to the breeding habits of tsetse flies, and the observations of many workers are noted. The chief characteristics of *G. palpalis* breeding places are shade, and a dry, loose soil, or dry vegetable debris. The sites chosen have usually water in the vicinity, but breeding places have been found over 1,600 yards from any discoverable water. Lloyd records *G. morsitans* breeding in such places as hollows in trees, beneath fallen dead trees or branches, in burrows of animals, etc., and points out that the one feature common to all the breeding places found was the presence of some relatively dark spot for the fly to conceal herself during gestation. Most of the pupæ were found in unmistakable relationship to native and game paths, fords, drinking places and salt licks. A similar selection of the vicinity of game paths is recorded by Lamborn for *G. brevipalpis*.

Fly belts, migrations, ranges of flight, proportion of the sexes are dealt with in about thirty-four pages. The experiences of many observers are recorded regarding the habitat of different species of tsetse flies, some divergence of opinion being expressed. Neave's interesting classification of East African *Glossina* according to environment is given.

(*a*) Requiring a great degree of atmospheric humidity:—
- (*a*) 1.—Requiring a high temperature, *palpalis*.
- (*a*) 2.—Not requiring a high temperature, *fusca*.

(*b*) Requiring only a moderate degree of humidity:—
- (*b*) 1.—Requiring comparatively little cover, *pallidipes*.
- (*b*) 2.—Requiring fairly heavy timber and bush, *brevipalpis*.
- (*b*) 3.—Requiring more or less dense forest, *austeni*.

(*c*) Independent of water and most active in a dry atmosphere, *morsitans* and *longipennis*.

The food of tsetse flies and their relation to big game take up about thirty

pages. The much disputed question of the dependence of *Glossina*, especially *G. morsitans*, on big game for their food supply is considered in this section, and the evidence and conflicting opinions advanced by advocates on both sides are quoted. In his summing up Major Austen says, "the close association admittedly existing between *certain* species of tsetse fly and big game, in parts of the African continent, must on no account be assumed to be those obtaining between game and *all* species of *Glossina* everywhere throughout their range . . . though there be definite grounds for considering big game as constituting a reserve of food enabling tsetse flies such as *G. morsitans*, *G. pallidipes*, and *G. brevipalpis* to subsist in a given region, there is as yet no scientific proof that the presence of these animals is absolutely necessary to the continued existence of these insects. Under these conditions the utmost caution would seem to be necessary before definitely advising the taking of measures which would have irreparable results."

To a consideration of destructive and prophylactic measures about forty pages are devoted. The predatory enemies of tsetse flies are dealt with at length, as a possible means of control, but as the authors say, much of what has been stated is evidently highly theoretical, and one imagines that few hope for any considerable reduction of *Glossina* from the enlistment of their predatory foes on the side of the sanitarian.

Catching and trapping of tsetse flies is not a practical measure of control over wide areas of fly infested country, but where *Glossina* are limited in their distribution good results have been obtained, the Island of Principe being a case in point. Here the catch of tsetse flies for October, 1911, amounted to 16,000, and it is stated that the bird lime method, combined with other destructive measures, achieved such success that after April, 1914, the offer of a reward equivalent to fifteen shillings for every tsetse captured failed to produce a single fly.

The provision of artificial breeding places reproducing the features of the natural sites selected by the insect has been advocated and employed to some extent, but obviously this method of control to be successful to any extent must be combined with clearing operations so that the pregnant females may be forced to resort to the trap breeding places.

Of the measures directed against *Glossina* clearing is by far the most efficacious, and such operations are regularly carried out around villages and settlements, along roads, in the neighbourhood of landing places, wells, etc.; but the primary aim of this clearing in most cases is the protection of man rather than any material reduction of tsetse flies. The authors reproduce Shircore's proposals for the limitation and destruction of *G. morsitans* by location and clearing of the primary and permanent breeding places from which the flies radiate and establish secondary centres. Unfortunately Lamborn's subsequent investigations do not support the practicability of Shircore's ideas as the primary foci appear far too extensive to be dealt with as proposed.

Ample directions are given for the collection, preservation and dissection of tsetse flies, and many other matters of interest and importance are discussed.

Enough has been said to show the nature and scope of this book; it presents in compact form the results of years of labour and the substance of a mass of memoirs which will be of infinite assistance to all concerned in any way in the campaign against the tsetse fly. The conflict of opinion in some of the papers quoted demonstrates in an impressive way the extent of the work and the investigations which still remain to be carried out.

Correspondence.

"*BACTERIUM PNEUMOSINTES*" AND THE CAUSE OF INFLUENZA.

TO THE EDITOR OF THE JOURNAL OF THE ROYAL ARMY MEDICAL CORPS.

SIR,—In view of the renewed attention now being given to this question not only by the great medical weeklies but also by the lay press, may I be vouchsafed a few supplementary remarks upon the subject? It would be gathered, from the references made, that the question of the organismal nature of the *Bacterium pneumosintes* of Olitsky and Gates is rapidly coming to be regarded as a *chose jugée*, the verdict having been given in its favour; whereas such a judgment would appear to be distinctly precipitate. In the present enthusiasm for filter-passing organisms there is, perhaps, a tendency to overlook the reasoned doubts which have been expressed by several workers, although no additional evidence of a conclusive character has since been brought forward, so far as I am aware.

There has been quite recently published a most interesting and important paper by Levinthal and Fernbach (*Zeitschr. Hyg.*, 96, part iv., November, 1922, p. 455) on the morphology of the influenza bacillus and the problem of the ætiology of the disease. These authors, after giving full consideration to the work of the Americans and others on filter-passers, nevertheless, *still* pin their faith to Pfeiffer's bacillus, and regard pandemics as being due to a greatly enhanced virulence of this organism. The additional factor (" Moment ") requisite at times in explanation is to be found in a " hochvirulente Mutation."

Though not stated in so many words, it is manifest that Levinthal and Fernbach are sceptical about *B. pneumosintes* being a true organism. Because they themselves refer to another most interesting paper, by Branham and Hall (*Journ. Infect. Dis.*, 28, 1921, p. 143), to which too little attention has been given in recent discussions in this country. Branham and Hall found that not only in Noguchi tubes which were inoculated with material from various secretions, *but also in those which were left uninoculated*, as control tubes, *the characteristic cloudiness developed after some time* in the neighbourhood of the piece of kidney-tissue, which is held to mark the " growth " of the filter-passing organism! This cloudiness, the authors found, was due to the presence of granules of various kinds which they regard as " artefacts " (in the sense that they are not living organisms) resulting most probably from *tissue-autolysis*. And they conclude that great caution is necessary in interpreting microscopic findings from such " cultures." It is indeed!

Professor M'Intosh briefly referred to this point in his address at the British Medical Association (*vide British Medical Journal*, August 19, 1922, p. 304), but he might well have stressed it much more. The only

specific reference to Branham and Hall's work that I can find as being made during the discussion was to their lack of success in cultivating a filtrable virus. Naturally, Branham and Hall taking (as I think, correctly) the view that they did about the non-living nature of the bodies they found, could only conclude that their search for a filter-passing organism had been unsuccessful. As I have recently indicated, there is not a shred of real cytological evidence—which is, above all, required—that these granules are themselves anything but the end-products of cytolysis. In the light of the above observations, a recent announcement by Olitsky and Gates that another "species" of filter-passer, regarded in this case as being non-pathogenic, can be "grown" from the secretions of healthy persons, must appear of very dubious significance. It is becoming more and more apparent that a *grave source of error* is inherently present in media of the Noguchi type, where the question is one of minute filter-passing organisms!

Levinthal and Fernbach point out that Pfeiffer's bacillus is the most regularly occurring micro-organism in cases of epidemic influenza. This is, indeed, becoming more and more recognized. Even the most recent worker, Lister, according to an annotation in the *British Medical Journal* (February 3), found the influenza bacillus in no fewer than fifty-three out of fifty-six cases in Johannesburg during the pandemic of 1918. This isolation-percentage would be considered by no means an unsuccessful one in the case of many organisms held to be established as the cause of particular diseases; yet this fact seems to attract little attention. On the other hand, in his latest search for the filter-passer, Lister obtained it in only five out of fifteen cases; but far more interest seems to be attached to this latter finding!

Now, in view of the fact that it is admitted that not only pure cultures of Pfeiffer's bacillus, but also its toxin can, on inoculation, produce results quite comparable[1] with the lesions, etc., which characterize influenza, I will venture to express a feeling of surprise that anyone should appear willing, as it were, to cast aside a known and well-established organism, in order to run after an extremely vague and nebulous "filter-passer"—until, at least, every effort has been made to probe the potentialities of Pfeiffer's bacillus.

Levinthal and Fernbach, in their paper, have carefully examined anew the possibilities of *Bacillus influenzæ*. And, to me, the most interesting point in their work is that, among the different strains isolated which the authors regard as being types of Pfeiffer's bacillus, they have found one which is actually *hæmolytic*; namely, a form corresponding to Pritchett and Stillman's *Bacillus X*. If this view of Levinthal and Fernbach's—that there is a "strain" of Pfeiffer's bacillus which is hæmolytic—is con-

[1] Levinthal and Fernbach, indeed, regard the "picture" thus obtained by Blake and Cecil, and Fildes and M'Intosh, respectively, as being, if anything, more true than that obtained by Olitsky and Gates after inoculation with their "*Bacterium pneumosintes*."

firmed, this may prove a most important point, and afford the additional "Moment" of enhanced virulence required. Now, if such a type is able in certain conditions to produce a hæmolytic ferment, this may be also capable of cytolytic action. And therewith contact is made with my own view as to the true nature of the pathogenic agent present in the successful cultures of Olitsky and Gates—successful, I mean, in regard to inoculation results. As I have recently indicated my view, it is unnecessary to refer again to it here.

I am, Sir, etc.,
H. M. WOODCOCK.

Notices.

EDITORIAL NOTICES.

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Journal of the Royal Army Medical Corps.

Original Communications.

"RICKETTSIA"-BODIES AS A RESULT OF CELL-DIGESTION OR LYSIS.

By H. M. WOODCOCK, D.Sc.Lond.
Fellow of University College.

(*Continued from p.* 97.)

THE INTRACELLULAR OCCURRENCE OF "*RICKETTSIA MELOPHAGI*."

IN the case of other sheep-keds from which I have preparations, yet another different phase is seen in the relation between the crithidial parasites and the host, which is of considerable biological significance.

In smears of the alimentary tract, there are, in the first place, numerous "Crithidiæ" present. These are practically all "free," and the absence of agglomeration-clusters and masses marks a conspicuous difference, as compared with the condition found in the first case above described. Further, the great majority of the parasites are free from granules; now and again an individual shows two or three in its cytoplasm, but that is all. In other words, the parasites in the lumen of the gut are, to all appearance, perfectly normal and healthy; but they are not present in anything like the overwhelming masses that occurred in the first case.

Dispersed more or less throughout the smear are numbers of typical "Rickettsia"-granules. And it is interesting to note that there is, as it were, a corresponding difference in the disposition of the granules and that of the parasites in the two cases, respectively. Here, the granules are much more uniformly scattered. There will be more in one field, fewer in another; but the bulk of them are separate, or at most in twos or threes. With the important exception noted below, there are not the masses and clumps of varying size, in which the granules tended to be aggregated in

the first case, to be seen here. The disposition in this case, in short, is much more comparable to that of the free granules in the Leptomonad culture above described. And just as manifestly as the masses of granules in the first ked resulted, at any rate very largely, from the disintegration of the parasites occurring at the time "free" in the lumen of the gut, so it is apparent that their origin here is not precisely in this same manner. Whence, then, do the free granules come in this case? At first sight it would be thought that there was no connexion whatever between the two things—"Crithidiæ" and "Rickettsias"!

Towards one end of the smear, fortunately, were left the teased-up fragments of the alimentary tract. The larger portions are simply blue-stained masses, in which nothing definite can be distinguished. But in the neighbourhood of these occur isolated epithelial cells, or groups of two or three still connected; and these are well stained in the customary differential manner. In addition, small broken-off or detached fragments of cytoplasm, *minus* the cell-nucleus, are scattered about everywhere. Now in, and in intimate association with, the cytoplasm of these cells, abundant "Rickettsia"-granules are to be found (figs. 15-17). I need only add that the granular elements in the cell-cytoplasm are indistinguishable in appearance from all the minute free bodies around; because it will be apparent, I think, from the figures, that we are dealing with the same thing. Some of the cell masses show, apparently, very few granules; but this is very largely, I consider, because everything is stained blue, since it is well known that in such cases the red element of the stain for some reason does not "act." But it is instructive to observe how, in the thinner "tails" of such masses, where the cell-cytoplasm has been streaked-out, as it were, the red granules come up prominently. In short, I have little doubt that many of the epithelial cells of the gut in this case contained "Rickettsia"-granules.

Next, as regards the actual situation of these granules. In dealing with isolated cells in smears, which are of course flattened out to a greater or less extent, one is placed at a disadvantage as compared with a study of sections in some respects, and care is necessary in one's interpretation; though, on the other hand, in such a study as this, I think that Giemsa-stained smears offer compensating advantages because the staining effects are, on the whole, so much more reliable and constant than in sections thus stained. I have scrutinized several cells and cell-masses very carefully and I think the correct conclusion is as follows. Needless to say, I am bearing in mind also the knowledge which has been detailed or summarized in the first part of this paper. I regard the cell shown in fig. 17 as presenting a condition such as would probably be frequently found in life. The cell is seen, broadly speaking, from the side, and its right-hand border represents the free face, i.e., that abutting on the lumen of the gut when in situ. At this side of the cell is seen a mass of "Rickettsia"-granules, especially dense near the edge. Now the outermost mass of granules could,

perhaps, be regarded as being not really intracellular, but closely applied or attached to this edge of the cell. I do not think this is the case, at least to any extent. In the first place, there is no indication of an arrangement in rows, such as is seen when "free" crithidial parasites lying parallel to one another have become resolved into granules. Moreover, I do not see why they should be so adherent to the cell-cytoplasm that no "trails" of separate granules are seen, set free when the smear was made (and this applies equally to all the cells, or detached pieces of cytoplasm showing the granules). These numbers of aggregated granules, in this smear, always occur in a cytoplasmic matrix. Even in the particular cell figured, the cytoplasm can be traced up to the border in the lower right-hand half; and in the upper half I think it is rendered indistinguishable only by reason of the packed mass of granules. Further, as one passes inwards, towards the cell nucleus, although the dense arrangement of the granules is lost, nevertheless it is seen that the cytoplasm does indubitably still contain the granules, although in this case more sparsely scattered. Again in figs. 15 and 16 the cell-cytoplasm contains numbers of the granules (the latter figure shows a portion of a fragment of the epithelium, consisting of three or four cells, somewhat streaked out). And any number of figures could be given, both of cells and of isolated or broken-off cytoplasmic fragments showing similar appearances. It could not be contended that, in all these cases, one happens to be seeing only a surface layer of granules attached to the free face of the cell on which one is looking vertically down! Lastly, in one interesting case, a portion of cell-cytoplasm shows at one place most minute granules just appearing in the cytoplasm and a gradual transition from these to the more conspicuous ones, of the customary size, a short distance away. I have considered this point in some detail just because I have no sections for comparison. But I conclude, unhesitatingly, that these granules are, at any rate as regards the great majority, actually in the cytoplasm of the epithelial cells of the gut; and that they tend to be most numerous, at times densely aggregated, in that portion nearest to the free border of the cell. These granules, then, represent, I consider, the intracellular "Rickettsias" which have been described in the ked.[1]

Now, how are these granules produced? They are formed in a manner entirely comparable with what we have seen to be the case in the first ked considered, namely, from the breaking-down of the "Crithidiæ." In the present instance, however, there cannot be any doubt, I think, that the disintegration is the result of the direct action of the epithelial cells. In other words, *the parasites have been digested by the cells and these granules are the end-products of the digestion;* they probably represent the unassimilable, "excretory" residue. I have already mentioned that the "free" parasites in this smear appear quite normal. The only broken-down individuals are found in close connexion with the epithelial cells.

[1] Certain of the figures given by Arkwright, Atkin and Bacot [1], of intracellular "Rickettsias" in the bed-bug, as seen in smears, markedly resemble my figures.

The course of the process, so far as it can be interpreted from the microscopical appearances, is as follows. In the first place, as is well known, many parasites tend to be attached to the cells. In the case of those about to be digested, this attachment becomes more intimate, so that the cytoplasm of the parasite (usually of the anterior half) is completely united with that of the cell, and indistinguishable therefrom, in the sense that one cannot say where the former ends and the latter begins (fig. 15, *a*). Next, this proximal portion of the parasite becomes lost—merged entirely into the cell-cytoplasm; and only the two nuclei and the distal half of the parasite remain separate and recognizable as such (fig. 15, *b*). I would ask readers to turn back to fig. 9 in the February number of the Journal, where these different stages in the absorption of the parasites are much more clearly seen. I could not give the full explanation of this figure at the time, because I wished first to consider the granules in relation to the cells. But now it will be understood, I think, that there we have simply small cytoplasmic fragments of epithelial cells, containing more or less numerous granules, in which this process of digestion was taking place at the time when they were isolated.

At a later stage, only the pairs of nuclear elements can still be distinguished in the cytoplasmic matrix (figs. 16 and 17, *c*); while finally, odd kinetonuclei, dispersed here and there, remain as the most persistent elements—those, apparently, most resistant to the digestion. The whole process, in fact, recalls irresistibly the progress of the lysis of the parasites agglomerated in clusters "free" in the lumen of the gut, in the first case dealt with.

I regard this union of the body of the parasite with the cytoplasm of the cell, and its gradual "indrawing" and absorption by the latter, as a form of ingestion. But I have seen no indication of the occurrence of complete crithidial individuals, as recognizable entities, within the cells; though, as will be gathered later, I do not think there is any reason why such cytophagy should not occur. But the appearances I have described point, in general, to the dissolution of the parasite and the absorption of the digested material taking place *pari passu* with its bodily incorporation into the cell-cytoplasm. In the course of the assimilation of this absorbed nutrient material, the granules are produced; just as, for instance, the platelet-granules are formed as a result of the digestion of blood-corpuscles and cells by the macrophages.

It may be pointed out that there is here a gap, which I have not filled; viz., that I have not shown the actual formation of the granules in relation to the parasites in course of digestion. That is, unfortunately, true; but the gap is unavoidable, just because of the merging of the body of the parasite in that of the cell; though the nuclear remains can be distinguished, as I have shown. In the case of blood-corpuscles and cells ingested whole, of course there is no difficulty in correlating the appearance of the granules, or the resolution of the disintegrating nuclear

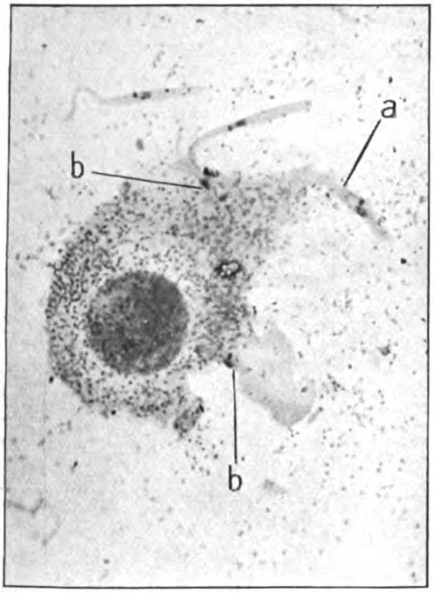


FIG 15

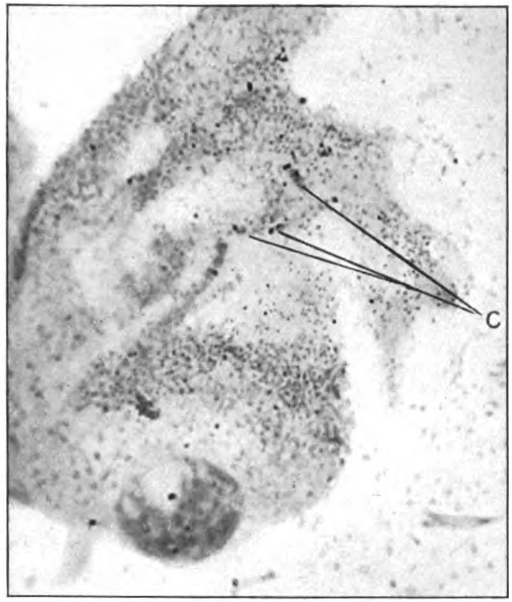


FIG. 16.

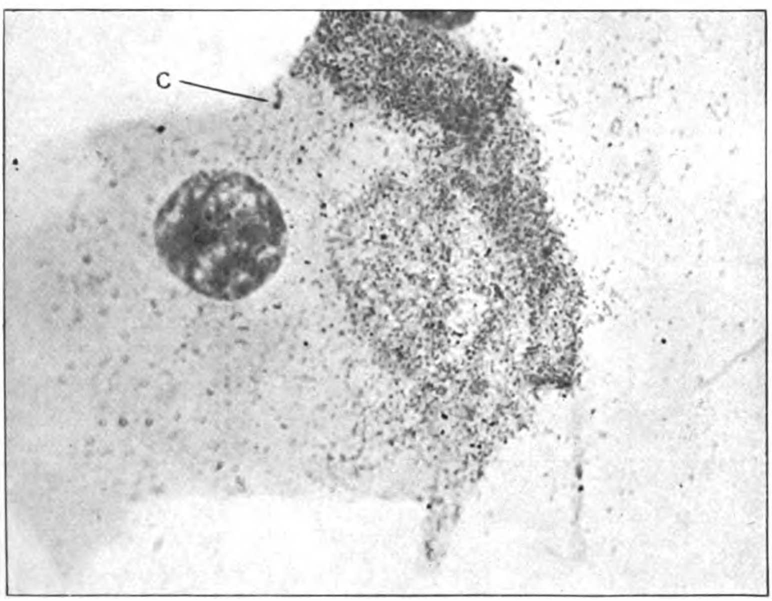


FIG. 17.

To illustrate "'Rickettsia'-Bodies as a Result of Cell-Digestion or Lysis," by H. M. WOODCOCK, D.Sc.Lond., Fellow of University College.

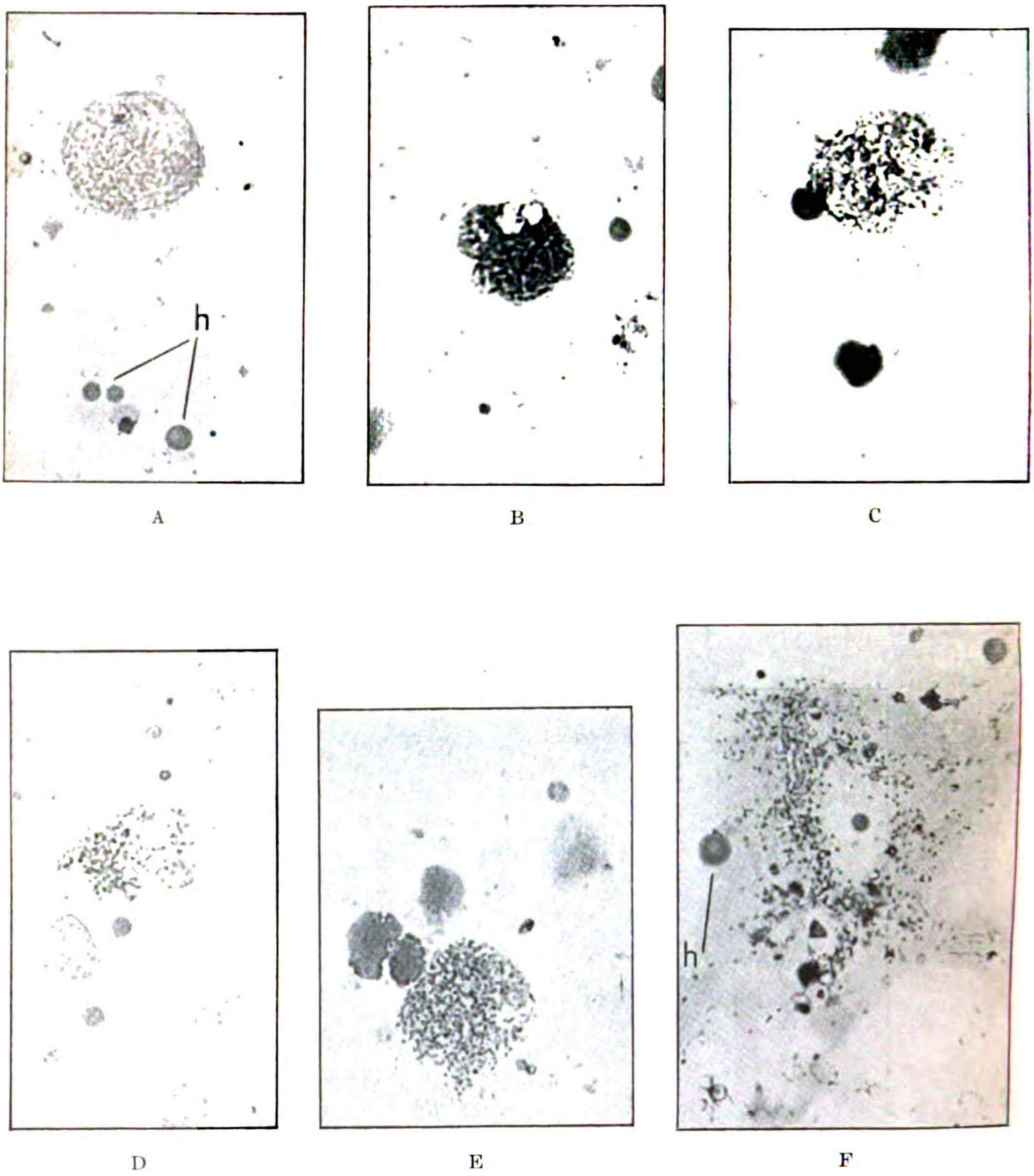


FIG. 18.

To illustrate "'Rickettsia'-Bodies as a Result of Cell-Digestion or Lysis," by H. M. WOODCOCK, D.Sc.Lond., Fellow of University College.

material into granules, since this can be observed occurring in situ. And, in bridging the above gap, I think I can claim that my observations of the unmistakeable production of perfectly similar granules in the course of the dissolution of the "free" crithidial parasites in the gut must be taken into account. Again, the appearance of these intracellular granules is strikingly similar to that found in many epithelial cells undergoing lysis, as I have quite recently shown [20]; the chief difference being that, in this latter case, the cell-nucleus itself is being destroyed. No, I cannot think that we have here an intracellular micro-organism! Indeed, for myself, I can only say that merely looking at fig. 16, for instance, is sufficient to dispel any idea that these granules are living organisms.

I consider, therefore, that the above is the true explanation, very largely, of the intracellular occurrence of *Rickettsia melophagi*, and that point No. 5, in my summary of our previous knowledge relating to this form, is therewith brought into line with my view. I say "very largely," because I wish it to be kept in mind that this is only *one* special case. Other forms of "Rickettsia" are not derived from the digestion or lysis of crithidial parasites, of course; but I think they can be, and are produced by that of corpuscular or cellular elements. And it is not unlikely, indeed, that *R. melophagi* itself sometimes originates in this latter manner also.

A general objection to the above explanation, however, may be raised by some to the effect that it is preposterous to suppose that epithelial cells of the digestive tract do "ingest" and digest solid bodies, such as these parasites, at all. Now, I think it will come to be recognized that the actual ingestion of solid, organized food material, prior to its digestion, occurs to a far greater extent than has been supposed. I give instances below of the ingestion and intracellular digestion of the blood-corpuscles (i.e., of hæmatophagy and hæmetaboly), thus corroborating Reichenow's work [10]. And, as regards these flagellate parasites, I can see no biological difficulty in the way of a similar occurrence taking place.

In the case of *Trypanosoma lewisi* in the rat-flea, as shown by Minchin and Thomson in their invaluable monograph [7], the parasites undergo a definite phase of their life-cycle inside the epithelial cells of the stomach. But, in the phylogenetic evolution of this specialization, it is extremely probable that the parasites were first destroyed and digested by the cells (even if we regard them, at one stage, as voluntarily penetrating therein); only in this particular instance, as so far known,[1] have they acquired

[1] There is another very interesting case of alleged intracellular multiplication, namely. that of *Crithidia leptocoridis*, from a box-elder bug, described by Miss McCulloch [6], I consider this authoress is entirely mistaken in her interpretation, and that she really observed stages in the intracellular digestion of the parasites. She describes large "cellular masses," or "plasmodia," which she regards as corresponding to "tailless spheres"—although she admits this explanation is somewhat doubtful, but the most plausible. Her figures bear no resemblance whatever to the usual picture of multiple division, and I think there can be no doubt that she was dealing with dissolution of the parasites in the epithelial cells. The nuclei themselves are shown breaking down in the common cytoplasm into minute granules!

resistance to the digestive ferments of the cells, and the capacity to continue their development in this situation. And even in the case of *T. lewisi* to-day it appears most probable, from the authors' account, that sometimes the parasites are not able to establish themselves intracellularly, but are destroyed and digested by the cells—because the occurrence of intracellular forms in course of degeneration and absorption is specially mentioned.

In the case of the ked-parasites there is quite another possibility, which can only be suggested here, but which seems to me worthy of consideration. It may be that, at any rate during certain periods, the keds live upon the parasites rather than on the blood itself! If this be so, all this mass of granules represents, really, fæcal material—unassimilable excreta. Various writers have commented upon the enormous crowding of the gut with the parasites—at times to such an extent that it is difficult to imagine how the food (blood) can pass backwards. Thus Swingle [17] says "they fill the digestive tube so full in the posterior two-thirds that it is a wonder how any food could pass through without crowding them out." And it is important to note that the ked is engaged more or less constantly in feeding, fresh blood being always present in the anterior part of the alimentary tract, when an individual is examined. Doubtless, these huge numbers of the parasites are themselves feeding (by osmosis) on the partly digested, or at least altered, blood, including dissolved hæmoglobin. And, in my own opinion, the granules in the parasites are connected with the metabolization (probably incomplete) of the hæmoglobin, just as those in the cells are related, I consider, to the digestion of the parasites themselves (cf. again the case of the platelet-granules—all-important in this connexion—which are produced as a result of the digestion both of hæmoglobin and of leucocytes, etc.). Therefore, the fresh blood continually being taken in may be used largely—I do not say entirely, of course—by the flagellates; and the ked itself probably indulges at intervals in a hearty meal upon the latter. If this be the case, the varying condition in which the parasites are found at different times (in the examination of different keds), would be accounted for.

In the particular ked which I have been considering above, I regard the numerous, though by no means swarming, normal "Crithidiæ," as representing a fresh influx, a replenishment of the general stock, after a preceding lot have been practically all destroyed and digested (cf. the condition in the ked from which figs. 10 and 11 are taken). This fresh invasion may be the result either of a new infection with trypanosomes from the sheep or of a passage forwards again of a few persistent forms in the hind part of the gut, followed by multiplication. Subsequently, these parasites in turn may be expected to fall a "prey" to the host, in one guise or another, either by "free" lysis or by intracellular absorption. It will be seen that some extremely interesting biological points bearing on the mutual relations of host and parasite are thus incidentally indicated.

Lastly, we are now in a position to answer the question: Whence do all the free granules come, which are dispersed throughout the above smear, together with the healthy parasites? *They have been liberated from the cytoplasm of effete and broken-down epithelial cells, which have been cast off into the lumen of the gut.* The periodic desquamation of old, functionally worn-out cells into the lumen of the digestive tract is, of course, a well-known feature in insects. At this point, I need only mention that, as regards the epithelium in the sheep-ked, both Swingle (loc. cit.) and Pfeiffer (loc. cit.) comment upon the extent to which this process occurs. Now, in my own smears, allowing for the probability that some of the isolated epithelial cells were broken loose in teasing up the gut, nevertheless, numerous fragments of cytoplasm are to be found in different parts of the smear—not only near to, but far away from the little pieces of teased-up gut—which certainly represent the remains of cells which have been naturally desquamated (cf. also fig. 9). As has been shown, many of these contain a quantity of the granules. And stages can be traced in the gradually increasing tenuity of such cytoplasmic fragments, indicated by the increasing faintness of the staining and the increasing difficulty of detecting the matrix in which the clumps of granules have originated. Ultimately, of course, when the cytoplasmic matrix quite disappears, the resistant granules left are dispersed amongst the others present in the lumen.

II.—THE PRODUCTION OF "RICKETTSIA"-BODIES BY KARYOLYSIS AND CELL-DIGESTION IN A BIRD-MITE (*DERMANYSSUS*, SP.).

ON THE OCCURRENCE OF INTRACELLULAR DIGESTION OF THE BLOOD.

As I mentioned in a former paper [21], Reichenow has found that, in the case of a blood-sucking mite (*Liponyssus*) ectoparasitic on lizards, there is intracellular digestion of the red cells, i.e., the normal hæmetaboly is there preceded by hæmatophagy. Further, this author also describes how the effete epithelial cells of the gut are cast off into the lumen and there break down completely. Moreover, Reichenow points out that this process of intracellular digestion is of general occurrence among the Arachnida. And this author himself even utters the following significant warning: "Man muss . . . daher die mannigfaltigen Bilder berücksichtigen, unter denen die halbverdaute Nahrung in den Darmepithelzellen ersheint, um auf der Suche nach Parasiten nicht in Irrthümer zu verfallen"![1] How much more necessary is this caution, therefore, when minute granular parasitic organisms are in question, in view of the occurrence of granular end-products of blood-digestion and of the lysis of effete cells—normally and pathologically.

[1] I am very much afraid that Reichenow, in his recent enthusiasm for "symbionts," has himself fallen into an error of this nature (*vide* footnote, p. 258).

A recent announcement by Sikora [15] that she has found "Rickettsias" in various bird-lice (Mallophaga) led me to look again over some preparations of the gut of a bird-mite (*Dermanyssus*, sp.), which I made during the course of my study on the blood-parasites of the chaffinch. These bird-mites occurred occasionally on the chaffinches, and I had examined a few to see if I could find any stages in the life-cycle of one or other of the avian hæmatozoa, but without success. Unfortunately, I did not make any sections; but even the examination of smears has proved most instructive, especially with having Reichenow's observations as a guide. As in the case of the keds, my smears from the mites were all stained by Giemsa in the usual manner.

FIG. 19.

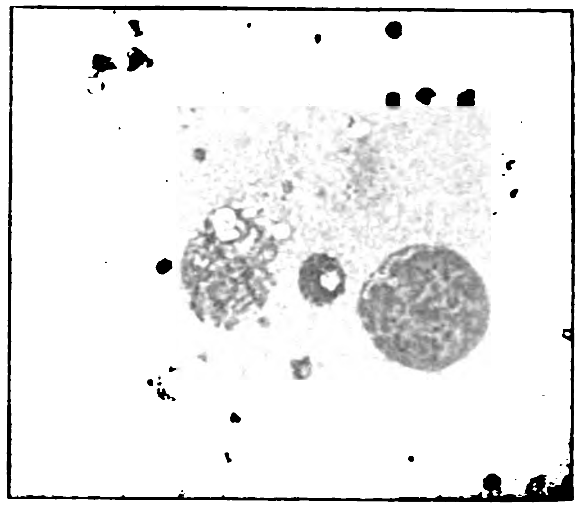
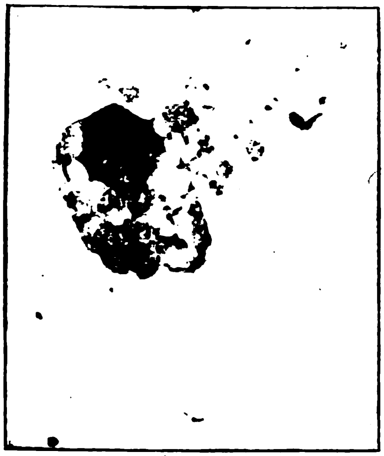


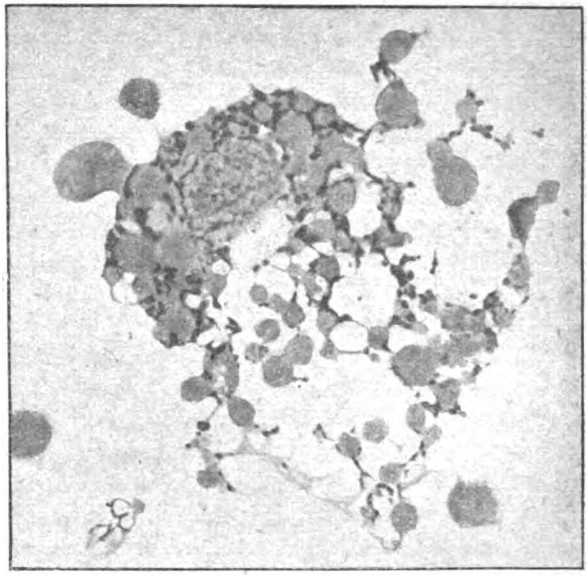
FIG. 20.

In the case of the bird-mite, there is a preliminary disorganization of the red blood-cells in the lumen before they are ingested. The cell loses its normal form and appearance and becomes a rounded mass; the hæmoglobin stains more deeply (probably because it becomes darker in colour), and the nucleus[1] becomes contracted and nearly round (cf. fig. 19). Next, the nucleus is cast out, or at any rate parts company with the globule of hæmoglobin. Some of the globules run together to form larger ones, while others break up into smaller ones; hence, numbers of globules, of very varying size, are present throughout the smear (cf. figs. 18 and 23, *h*). The cell-nuclei—those, that is, which do not happen to become ingested—gradually break down and disappear. Whether, and if so, to what extent there is complete dissolution of the hæmoglobin, while free in the lumen,

[1] It will be remembered that the red cells of birds are nucleated.



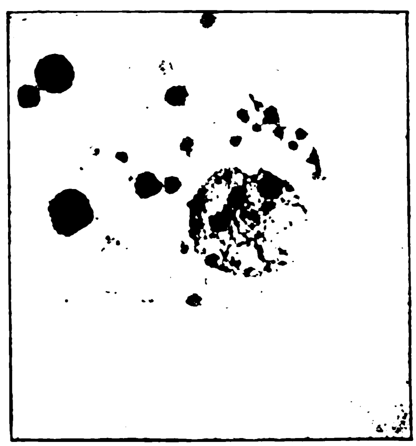
A



B

Fig. 21.

A



B

Fig. 22.

I am not able to say. But at any rate large numbers of the globules are ingested and then digested by the epithelial cells.

In fig. 21 A and B are shown two epithelial cells, containing numerous globules of hæmoglobin, of varying size, and also many pigment grains; these latter appear of a very dark brownish-green colour. Unfortunately, in making the smear, the cells laden with these bodies tend to rupture, as has been especially the case in B. The condition presented in both cases is clearly that of a cell which has produced the pigment as a result of a preceding meal of "corpuscles," and has subsequently gorged itself anew, the fresh globules of hæmoglobin being sandwiched in amongst the pigment grains. I think it is worth while here to give Reichenow's account in his own words :—

"Die Epithelzellen der Darmwand sind von sehr verschiedener Grösse; die grösseren, an der Peripherie kolbenförmig verdichten, überdecken die kleineren vollständig. . . . Wenn sich der Mitteldarm mit Blut gefüllt hat, dann bilden die grössten kolbenförmigen Zellen Pseudopodien aus, mit deren Hilfe sie sich die Eidechsenblutkörperchen einverleiben. Haben sie sich mit diesen angefüllt, dann werden die Pseudopodien eingezogen und die Blutkörper werden verdaut; als unverdauliche Reste speichern sich grosse, braune Pigmentkörner in der Zelle auf. Nach beendeter Verdauung setzt die Phagocytose von neuem ein und wiederholt sich so oft, bis die Zelle mit Pigmentkörnern gänzlich vollgepfropft und damit zu weiterer Thätigkeit unfähig ist. Die verbrauchten Phagocyten lösen sich aus dem Epithelverbande los und schwimmen dann frei im Darmlumen, wo sie zum Theil zerfallen. Gegen Ende der Verdauung ist der Mitteldarm von grossen Massen dieser abgestossenen Zellen erfüllt."

I need only say that my own observations corroborate the above description. In regard to the pigment production, it may be added that I have found many stages[1] and have come to the conclusion that the pigment is gradually formed *in situ*, where the ingested little mass of hæmoglobin lies. It represents a gradual separation of unassimilable residual matter as the nutrient material is being digested and absorbed. The colour gradually changes from a lighter yellowish-green to a much darker brownish-green, and there is concurrently a distinct contraction in size of the little mass. Finally, as Reichenow says, many effete cells are seen packed with nothing but these dark pigment grains. The impression I have gained is that cells showing this type of hæmetaboly take up mainly, or perhaps exclusively, hæmoglobin. And I should say that, in general, no red-staining granular elements are formed, *normally*, in such cells as a result of this digestion. It must be remembered that most of this digested nutriment, at all events, will not be actually assimilated by the cell itself,

[1] Other stages in this process of digestion might with advantage have been figured, but as it is the Editor has most kindly allowed me to have more blocks than usual to illustrate this paper.

but passed on for the use of the body. Moreover, the course of the hæmetaboly here differs from that in the case of macrophages, for instance, where no pigment is as a rule produced.

In the case of other cells, however, the matter is different. Many cells are to be found containing ingested nuclei and nuclear material (figs. 20 and 22 A and B). I have not found any cells containing both nuclear material and manifest hæmoglobin, recently ingested. Neither have I ever seen pigment grains in these cells containing nuclear material in course of digestion. But now and again "vacuoles" are present which remind me strongly of the pallid, corpuscular "vacuoles," indicating an early stage in the digestion of hæmoglobin, found in many cases previously considered. It looks as if cells of slightly differing physiological character or function are respectively concerned in these two varieties of hæmatophagy and cytophagy.

The nuclei thus ingested are of very varied character. Many of them, especially the smaller ones (fig. 22), are the nuclei of the bird's red cells,[1] others those of its leucocytes; but also healthy, still functioning cells may undoubtedly eat the nuclei of effete epithelial cells, which have been desquamated (cf. fig. 20). And as end-products of this digestion of nuclear material, numerous red-staining granules, of the type with which we are becoming so familiar, are left over in the cytoplasm.[2] To these I refer subsequently.

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[1] Numerous red-staining masses of varying size and usually slightly irregular in outline occur free all over the smear. These are mostly the nuclei of still unaltered (undigested) red blood-cells, which have been liberated and squashed in making the smear. A perfectly similar appearance is of common occurrence in any smear of bird's blood; the red-cell nuclei being very liable to become thus distorted. Others are those of the leucocytes, or of epithelial cells themselves, which have been broken loose and been artificially destroyed, also at the time of making the preparation. The nuclei of younger epithelial cells are much smaller than those of the older ones, as is pointed out also by Reichenow.

[2] I may point out here that this process of the digestion of nuclear material strongly recalls the corresponding process in the case of the macrophages, as I described in my first paper [19]. And I should like to take this opportunity of briefly referring to, and acknowledging the importance of, two earlier papers to which my attention has been drawn since publishing my own. One is an account by Low and Wenyon [5A] of cell inclusions in leucocytes, in which the earlier stages of hæmatophagy of nuclear material by cells of endothelial character are excellently illustrated. The other is an account by Sir W. Leishman [5] of cell inclusions in the blood, in a case of blackwater fever, the cells concerned again being of mononuclear (endothelial) type. This is a most interesting and suggestive paper, correlating the granules and "bodies" of the "Chlamydozoan" diseases with the appearances found in the macrophagic cells. But I do wonder if the author would still prefer his explanation to mine? His fig. 15 shows beautifully the appearance of platelet-granules in the cell-cytoplasm; and, indeed, Leishman quotes from a paper by Christophers and Bentley, published as long ago as 1908, in which these authors themselves comment on the resemblance of such included granules to blood plates. These earlier fragmentary observations point irresistibly to my mind in the same direction regarding the origin of all these granular elements.

Another instance of intracellular hæmetaboly is undoubtedly furnished by the rat-flea (*Ceratophyllus fasciatus*); and this case is most important as showing that *in insects also* hæmatophagic behaviour occurs. I am really indebted to my friend, Dr. D. J. Thomson, for bringing this instance to my notice. When telling him on one occasion about Reichenow's work, he said that this brought to his mind certain observations recorded by Minchin and himself in their account of the rat-flea and *T. lewisi*, which he thought might have related to a comparable intracellular digestion of the red corpuscles. We have considered several of the figures again from this point of view, and I am glad to be able to add that he is confirmed in his opinion, with which I entirely agree.[1]

I need only refer those readers who are interested in this question to the following figures of the authors. Fig. 128 shows a large epithelial cell containing in its cytoplasm, in addition to multiplicative spheres of *T. lewisi*, numerous red corpuscles and fragments of corpuscles, some of them being in manifest digestive vacuoles. In fig. 142 is seen a large mass of hæmoglobin, resulting from the "running together" of several corpuscles. In this case, also, the authors describe the desquamation of effete cells into the lumen of the gut; and in fig. 135 such a cast-off cell again contains numerous corpuscles and corpuscular fragments. That the course of the intracellar digestion of the hæmoglobin probably follows in the main that shown to occur in mites is apparent from figs. 136 and 141, where darker, brownish-green pigment masses and grains are seen in the cytoplasm. Now the cells depicted in figs. 100 and 101 contain red-staining granules which might quite legitimately be regarded as "Rickettsias"; allowing for the magnification given (× 2,000), they vary from 0·3 micron to 0·6 micron in diameter.

Lastly, a most interesting point is that the egg-cells (ova) of the mite also ingest this solid, organized material as nutriment. Reichenow mentions (and figures) the remarkable occurrence of the ova in the gut-wall, amongst the epithelial cells. Such ova, isolated in making the preparation, occur not infrequently in my smear. They are readily distinguishable both by their spherical form and by the intense blue-colour which their cytoplasm is stained. And in a few cases, especially where the ovum has been slightly flattened or squashed, enabling its contents to be more clearly seen, I have found ingested nuclear material in the cytoplasm in course of digestion; in one or two instances, indeed, I have seen globules of hæmoglobin also included. And I think that in this manner the occurrence of *C. melophagia* in the eggs of the ked, which was noted

[1] It is remarkable that this explanation of the "yellow bodies" does not seem to have dawned even upon such a renowned worker as the late Professor Minchin (who was responsible for the minute histology); and I mention the point particularly, because it shows so admirably how fixed in our minds had become the erroneous idea that the tissue-cells of the Metazoa (with a few exceptions among very lowly forms) are unable to ingest and digest solid organized material.

by certain early workers, is to be explained. Far from the matter being one of hereditary infection, the parasites were most probably being calmly digested by the ova!

As regards the case of the mite, there may well be a correlation between the situation of the ova in the intestinal wall and their ingestion of blood-elements, etc. I allude to this point because it has, in my opinion, a highly significant bearing upon the question of normal hæmetaboly in relation to the production of reserve food material for the use of the embryo, which is stored up in the form of yolk or vitellus, in the case of vertebrates. That there is a great field of study in this connexion alone I am convinced.

Karyolysis and "Rickettsia"-bodies.

The disintegration or karyolysis of the nuclei of the old, worn-out cells of the gut of the mite, which have been desquamated into the lumen, results in the production of very definite "Rickettsia"-like elements. As has been indicated, isolated epithelial-cell nuclei are scattered about through the smear. Where these are of normal appearance, they may have been liberated in the course of making the preparation; but where the nuclei themselves are seen undergoing lysis, the associated cytoplasm of the effete cell has been, doubtless, already dissipated, and its contained mass of pigment-grains scattered. As in so many other cases, the nuclear material seems to be the most persistent and the last to break down.

A normal nucleus, still possessing its regular, unbroken contour (fig. 18. A) appears much the same as it does while in the normal cell (cf. fig. 20). But as it breaks down, the nuclear material as a whole gradually becomes resolved into a granular mass (fig. 18, B and C) (compare my account of the karyolysis of dying epithelial cells [20]). At first some of the granules are more sharply defined than others. The impression obtained here, as in the former case, is that the granular material is, as it were, solidified and hardened out of the more fluid, amorphous karyolymph, as this becomes altered.[1] At E is seen the end-stage in the process—an aggregation of quite discrete granules, having a coccal or diplococcal form. Finally, these granules become dispersed (F), and can be found scattered about everywhere. Of course, in this particular case, they are nothing like so numerous as those associated with the lysis of the masses of crithidial parasites in the sheep-ked. The size of these granules varies from 0·3 micron to 0·6 micron in diameter, in the coccal forms, and up to 0·8 micron or 0·9 micron in length, in the diplococcal forms. On seeing a field containing such clumps of granules, would not many

[1] This natural resolution of perfectly discrete particles out of a liquid colloid (the karyolymph) has an artificial parallel in the results of the action of fixatives on, for instance, egg-albumen, as shown by W. B. Hardy (*Journ. Physiol.*, 24, 1899, p. 158), to whose paper Professor C. J. Martin has very kindly drawn my attention.

enthusiastic believers in the "Rickettsias" say that here we have a "scanty" infection with them? In one of the many cases among Mallophaga ("bird-lice") in which Sikora (loc. cit.) found "Rickettsia"-like micro-organisms, she states that they lay "zum Theil als Wall, um einen Federdetritusklumpen"; just where we should expect them to be if these bodies were the non-assimilable products of the digestion of the material of the feathers.

I think, therefore, that along the above line is to be found the explanation of the alleged hereditary transmission of *R. melophagi*, which has been stated by Jungmann and Sikora to occur, because they found "Rickettsias" in young keds, just emerged from the egg, which had not

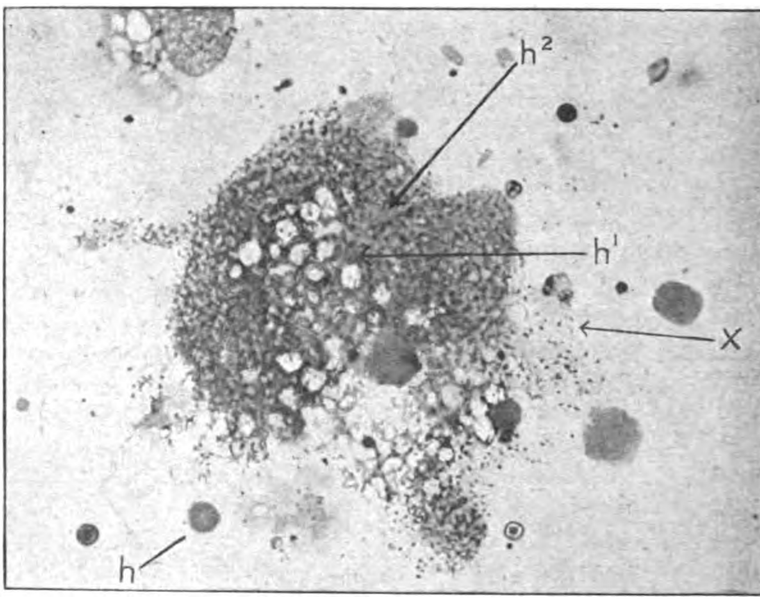

FIG. 23.

yet sucked blood. It is well known that cell-destruction and "absorption" take place to a large extent during the early life of an insect. And in the course of this cell lysis, granules similar to those which have been described will be produced in greater or less number. This, therefore, is my answer to point No. 6, enumerated in the first part of this paper.

Finally, I have observed indications of a most interesting occurrence, which I regard as pathological; and I think this has a significant bearing upon the question of what is happening when the "Rickettsias" are produced, for instance, in diseased conditions of the louse. In no case of a cell containing globules of hæmoglobin and pigment-grains, which was, to all appearance therefore, still functioning normally, have I seen *intra-nuclear* globules of hæmoglobin. But in several isolated nuclei, which show

early signs of alteration, globules of hæmoglobin have undoubtedly become included in the nucleus; now and again the original colour is still retained, and here and there a faint trace of colour still persists before the hæmoglobin has become completely bleached. Therefore I should say that one of the earliest "symptoms" of a digestive cell of this type becoming effete is that its nucleus "takes up" one or more of the globules of hæmoglobin which have been already ingested by the cytoplasm. But the nucleus is unable to digest this material in the customary manner, i.e., with production of pigment. The changes that take place recall strongly to my mind those which I found to occur when platelet-granules are produced in the large mononuclears as a result of the digestion of corpuscles. The little globule of hæmoglobin first becomes colourless, appearing like a "vacuole." Fig. 18, B, shows two such hæmoglobin-vacuoles in a nucleus; and in the large epithelial cell containing included nuclear material (fig. 20), on the left is seen an ingested cell-nucleus, undergoing karyolysis, which also possesses one of these hæmoglobin vacuoles.

Ultimately, in these little "vacuoles" appear red-staining granules and rodlets, the result of the modified course of the digestion. The large breaking-down nuclear mass in fig. 23 shows a most instructive condition. Three nuclei appear to be represented, in different stages of karyolysis. Probably the whole mass is the nuclear material of one greatly hypertrophied epithelial cell.[1] One of these nuclei is much larger than is usually the case and constitutes the irregular, oval body forming the left-hand half of the mass. Another, of more usual size, is seen at the right-hand side; while, below this, are the remains of a third nucleus, more completely disintegrated than the others. Around the periphery of the mass can be seen the granular elements into which it is becoming resolved, here and there somewhat "streaked-out" or separated, probably in making the smear. The general colour, throughout, is of course the usual red or slightly lilac. Now, included in this mass of nuclear material are numerous "vacuoles" of the nature above described, and, in addition, at least two globules of hæmoglobin. One of the latter (at h^1 in the figure) is still unaltered, the other (h^2) is much paler, having partly lost its colour. Most of the colourless "vacuoles" contain little granular bodies, which have frequently the form of short rods, now and again slightly curved. Unfortunately these do not come out well in the figure, but a few scattered ones which have been liberated are seen at (x). These inclusions resemble, also, those found in certain cases in the Kurloff-bodies in miniature, as it were. And in no case have I found any pigment-grains associated with this form of the metabolization of the hæmoglobin.

The important point, therefore, is this. Quite apart from the production of "Rickettsia"-like granules by the digestion and karyolysis of

[1] Minchin and Thomson (loc. cit.) figure such a huge, hypertrophied cell, with no fewer than five nuclei, just about to be cast off from the epithelial wall (fig. 128).

nuclear material, if the metabolization of the hæmoglobin takes what appears to be an unusual or abnormal course, "Rickettsia"-like, red-staining granules and rod-like elements are formed.

III.—THE GENERAL QUESTION OF THE "RICKETTSIAS."

Now, of course, it would not be justifiable to assert positively that, because one definitely alleged "Rickettsia" is, in reality, not a living organism, neither are all the other alleged forms. Nevertheless, I think it will be agreed that the observations I have above detailed must give even the most pronounced believer in these bodies as micro-organisms "furiously to think"; because the ked-form is, admittedly, *a typical "Rickettsia."* While, therefore, I am not in a position to say with certainty that all the other "species" are formations similar in origin and character, I do most strongly consider that such is, indeed, the case; and that what I have found in blood-sucking invertebrates goes far to support the view I have already outlined as to the nature of the "Rickettsias" described in cases of disease, e.g., typhus. I propose here to try and summarize my reasons, which can be grouped, roughly, under two headings: (1) the "Rickettsias" as a whole; and (2) the significance of the pathogenic forms.

THE "RICKETTSIAS" AS A WHOLE.

Bodies which have been regarded either as definite species of "Rickettsia" and named accordingly, or placed in this "group" as being of similar type, are now known to be of wide spread occurrence among ectoparasitic Arthropods. The following list may not be, indeed, quite complete: human lice, also a goat-louse (*Linognathus, vide* Hindle, 1921), various fleas, the bed-bug, the sheep-ked, various Mallophaga ("bird-lice"), at least one tick (*Dermacentor*), and a bird-mite (*Dermanyssus*). With the exception of the "bird-lice," these all digest blood as their food, and, indeed, one or two of the latter also now and again take blood (Sikora). In all these cases the bodies occur naturally in the digestive tract; occasionally, also, elsewhere. Frequently they are present in vast numbers, necessitating at times the use of the favourite German adjectives "kolossal" and "ungeheure." Notwithstanding this general occurrence, they are not found, normally, in any of the animals on which the above "hosts" feed; this point is emphasized by several authors. Only in the two or three human diseases associated with them have comparable bodies been described.

The "Rickettsias" are generally classed with the bacteria rather than with the protozoa, but they are remarkable in differing from most, at any rate, of the ordinary known micro-organisms in the following respects. They either will not stain, or else they stain only feebly with the usual aniline dyes, one or more of which invariably stain bacteria

intensely; but, on the other hand, they stain readily with Giemsa. Here also, however, they stain differently from bacteria, being usually of a bright red to lilac tint, instead of the intense purple, or even blue-purple colour of most bacteria when similarly stained (*vide* also Rocha-Lima [11]). The second point of difference is that the "Rickettsias" can only be cultivated with the greatest difficulty. Where indubitable "success" has been obtained, as for instance in the case of the ked-form, to which I may add my own case from *Ceratophyllus*, either crithidial or leptomonad parasites have also been associated in the culture-tube; and in both these cases, as we have seen, the "Rickettsia"-granules are being produced by the breaking-down of the flagellates.

The "Rickettsias" must be, I think, regarded as a whole, i.e., as all representing the same type of thing. Just as I should consider it not reasonable to suggest that in the ked or the rat-flea there are two things present—true "Rickettsia"-organisms and granular end-products of digestion and lysis—so I can see no valid reason for saying now that while certain "forms" must be relinquished, nevertheless the typhus and trench-fever types, for instance, remain genuine living organisms. Because, in the first place, just as they all differ from bacteria, so they *do* agree, among themselves, in exhibiting the particular differential characters which thus distinguish them. Again, admittedly, they are all very similar in type, size, morphology, and so on. Coccal forms, diplococcal forms, at times short bacillary rods—these are the appearances very generally presented. Rocha-Lima has carefully compared [17] the ked-granules with two forms from the louse, and beyond a very minute difference in size apparent from his figures (I doubt if he actually mentions this) finds no difference in character in the two cases. I venture to say that he certainly never thought he was dealing in one case with an organism, but in the other only with granules! Indeed, why should he have thought so?

How *are* these bodies to be distinguished in the two cases, as being either micro-organisms or end-products of digestion respectively? The separation cannot be made on morphological grounds. Quite as marked a variation or "pleomorphism" can be observed in many types of bacteria. Indeed, except in those cases where thread-forms have been described, pleomorphism is not at all excessive. However, in the form described by Arkwright, Atkin and (the late Mr.) Bacot [1] from the bed-bug the matter is rather different. Here long, curved or wavy threads, many microns in length, are included with minute coccal forms, etc., as being "phases" in one and the same "life-cycle." I think these authors are quite right in thus connecting these different forms; I entirely agree with them there. The only trouble is, I think they all indicate not a parasite, but a modified form of metabolization of the hæmoglobin, of a kind similar to that which takes place in the formation of the Kurloff-bodies, where, equally, long, coiling threads may constitute one of the varied types of inclusion to be found. (Cf. H. C. Ross, "Induced Cell-reproduction and Cancer," vol. ii, pl. 9,

facing p. 112). The faint greenish hue noted at times in the vacuoles in the Malpighian cells seems to me most suspicious of hæmoglobin. As for the occurrence of the bodies in the eggs, I have pointed out above that ingestion of nuclear material takes place in the eggs of the mite, and that it is there "broken down"; and as for their occurrence in the organ of Berlese, the digestion of spermatozoa is known to take place there, and it is interesting to note that the authors found very few thread-forms in this situation, but mainly small forms of the more usual type, representing, I think, a more customary course of the digestion. I confess I do not see how one is going to eliminate these end-products of digestion! On the other hand, I also confess that I find it difficult to understand how a micro-organism, if this were one, could be normally parasitic (in all bugs) in such numbers, in so many different types of cell, without being very harmful to the host. Yet the bugs do not appear to suffer any inconvenience therefrom.[1]

I have felt obliged to refer to this type found in the bed-bug, because the different forms presented are closely paralleled by those met with in the case of *R. prowazeki*, the typhus-form itself. The occurrence of thread-forms in the latter is commented upon by many workers; and, especially Wolbach, Todd and Palfrey [18], among their beautiful illustrations, figure (fig. 13) just such a tangled skein of "Rickettsia"-threads in a cell of an infected louse as is shown in certain of Arkwright, Atkin and Bacot's figures of the cells of the Malpighian tubules. Moreover, the American authors find that the pleomorphism of *R. prowazeki* is very similar to that of the type in the bug. They accept all these "phases" unhesitatingly, in both cases; and if they do, it is certainly not for me to quarrel with their interpretation. Here, too, I consider they also are right! Personally, however, I rather think I should not like to find an organism showing quite

[1] It might be contended that in this case we have to deal with one of the characteristic "symbionts" known to occur in many Insects. Now, while there is no doubt that true symbiotic organisms, of yeast-like nature, are present in the "mycetome" of various insects, I have little doubt that, in the case of blood-sucking Invertebrates, a number of false "symbionts" have been described, especially in relation with the digestive tract, which are not organisms at all. I will briefly refer to one or two cases. Reichenow, some time ago (*Arch. Protistenk.*, xx, 1910, p. 251) described such bodies from a leech (*Placobdella*). The cells of the œsophageal gland are packed with curved, thread-like bodies, which, here again, markedly resemble one form of inclusion in the Kurloff-bodies. In this account, Reichenow says that these bodies "contain no chromatinic elements," as is, indeed apparent from his figures, and that "there cannot be the slightest doubt that they are cell-products." But in a more recent paper (*Arch. Protistenk.*, xlv, 1922, p. 95), with his mind fixed on "symbionts," he now claims them as organisms and says that the reason he protested so strongly against this view previously was because he had considered only Protozoa and taken no account of vegetable organisms! But vegetable cells, equally with animal cells, must contain chromatin. "Bacteroids" that possess no chromatinic elements cannot be regarded as Bacteria. This point cannot be too strongly emphasized. Dobell, as a result of his study on the cytology of the Bacteria, came to the following conclusion (*Quart. Journ. Micr. Sci.*, lvi, 1911, p. 418): "All Bacteria which have been adequately investi-

such an amazing variety of forms in its *normal* environment; it would be a different matter in cultures. But, as indicating end-products of different modes of hæmetaboly, this varied appearance does not trouble me at all. However, the point is this; there is not the slightest ground for saying that, in the one case, an organism is concerned, but in the other, only "bodies." Whatever they are, they are the same type of thing!

Neither, of course, does cultivation assist us in separating the true from the false. Indeed, rather the contrary, because it so happens that just those "forms" which have been indubitably[1] "cultivated" are not organisms—at least, certainly not in my opinion. In short, I must repeat the "Rickettsias" are to be regarded as a whole; some cannot be taken and the others left.

For some time it was considered that only those lice which had fed on persons suffering from, or who had fairly recently suffered from typhus or trench-fever, contained "Rickettsias"; i.e., that normal lice, fed only on healthy persons, were always free from these "parasites." In view of the number of blood-sucking Arthropods which do customarily show these bodies, I think it would be rather strange if lice should not naturally contain them, at any rate, at times. And recent work has tended to establish that "Rickettsias" do occur in lice which have not fed on patients who had been attacked previously by one or other of the above diseases. Even if it be admitted (for the sake of argument) that there may

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gated are—like all other Protista—nucleate cells," i.e., they contain chromatin (Dobell regards the diffuse condition of the chromatin, as often found, as a nucleus). Quite apart from this, many of the figures Richenow gives (loc. cit.) of these bodies, both from the leech and from a mite, do not look to me even to have the appearance of living organisms at all. Again, many of the figures of what Stuhlmann regarded as "symbionts" in *Glossina* are manifestly the altered and partially digested nuclei (often ladder-like) of Trypanosomes, etc. Where these bodies, occurring in special cells connected with the digestive tract, have a definite and characteristic form, I think there can be no doubt that, in many cases, they are the manifestation of a particular form of the "digestive" function. And I agree with Reichenow and Roubaud that this may be to facilitate or inaugurate the early changes in the alteration of the ingested blood. Modifying the view of these authors, I consider these bodies may have "adsorbed" on to them the requisite enzyme, and by their liberation periodically into the lumen may thus facilitate the metabolization of more central portions of the blood, farther removed from the immediate influence of the enzymes in the cells of the gut. We must not lose sight of the all-important case of the Kurloff-bodies! The more one considers these, the more likely it appears that their production by the lymphocytes of the guinea-pig is also the manifestation of some particular normal function, exercised in this manner in this animal.

[1] Wolbach, Todd and Palfrey state that Kuczynski claims to have cultivated "*R. prowazeki*," This worker employed a medium of citrated blood-plasma to which was added blood treated with dilute sulphuric acid and neutralized, in order to supply products of protein decomposition! The "cultures" were grown in collodion-sacs in the peritoneal cavity of guinea-pigs. The American authors do not seem too confident that real success was obtained. But if it was, I should expect an increase of granular elements as a result of the metabolization of the "broken-down" hæmoglobin by the enzyme concerned.

be an element of doubt about, for instance, the cases described by Brumpt [2], and, further, that the extracellular type, known as *R. pediculi*, described by Rocha-Lima [12], is not above suspicion as being, in some cases, really identical with "*R. quintana*," the type associated with the trench-fever virus; nevertheless, there can be, I think, no doubt that the form described by Weigl as *R. rocha-limæ* is purely a louse-type. This form, although it occurs intracellularly and closely resembles *R. prowazeki*, producing also thread-forms, appears undoubtedly to occur in lice which have never fed on typhus patients. Therefore, I think it is quite likely that a purely extracellular type like *R. pediculi* may indeed occur, with no necessary connexion with trench-fever. Hindle (*Parasitol.*, xiii, 1921, p. 152) is also of the same opinion.[1]

Rosenberg [13] has recently given some most interesting details about *R. rocha-limæ*, including in his account a translation of parts of Weigl's Polish paper. All the workers who have studied this form regard it as causing an epidemic among the lice which is extremely contagious. Merely bringing lice infected therewith into the same room where there was healthy, non-infected stock, would suffice to infect the latter. Dust contaminated with infective fæcal material is incriminated as the agency of transmission. Certain points mentioned by Rosenberg are of considerable importance for my present purpose. In the first place, the infected lice do not appear to be able to completely digest their food (the blood); in other words, they suffer from indigestion! But, in spite of the overwhelming numbers of the bodies which occur in masses both inside the cells, causing these to swell up greatly, and in the lumen of the gut, they "übten keine toxische Wirkung auf die Laus aus. Die Zellen, die mit *R. rocha-limæ* überfüllt sind, erfüllen ihre physiologischen Funktionen weiter." I take this to mean that the "parasites" do not kill the louse; at any rate, as quickly as might reasonably be expected from the state of affairs apparently existing in the cells of the gut, if they were indeed micro-organisms. In this case, however, the blown-out cells are said not to break loose from the wall into the lumen, as in typhus-infected lice. Infected lice were allowed to feed on persons who had never had typhus and these suffered no inconvenience.

It is clear, therefore, from the above that we may have two manifestly similar conditions in the louse, one of which is inseparably associated with an infection with the virus of typhus, while the other is not.

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[1] In proof-correction, I am able to add a brief reference to a paper by Mello and others (*Journ. Trop. Med.*, January 15, 1923, p. 24). In North Portugal, the authors found "Rickettsia"-bodies in a small percentage of normal lice taken at a place which has never had cases of typhus, which were of such typical character that they could not decide, from smears, whether they were dealing with *R. prowazeki*, *R. pediculi*, or another! Moreover, these workers also found "Rickettsias" in normal *Phthirius pubis*.

THE SIGNIFICANCE OF THE PATHOGENIC "FORMS."

We have seen that a large number of ectoparasitic Arthropods normally contain peculiar bodies in the lumen of the digestive tract, sometimes in the epithelial cells of the gut-wall and also sometimes in those of other organs (e.g., in the case of the ked and the bed-bug), which all appear to be of the same fundamental character. Such variations as may be (but are not by any means invariably) exhibited are neither essential nor exclusive ("specific"), and I consider that they can be readily explained on two grounds: (*a*) the action of slightly differing enzymes in the case of different types of cell, or even in the case of the same cell-type on different occasions, upon (*b*) different kinds of organized material, such as hæmoglobin, nuclear material, parasites and so on. In certain of the forms met with in various fleas, for instance, some of the "Rickettsias" may well result from the digestion of leptomonads, different stages of trypanosomes, etc., as in the case of the sheep-ked. Even in the case of the louse, *Leptomonas pediculi* (Fantham) must not be left out of account in this connexion, although this parasite does not appear to be of very common occurrence. Brumpt (loc. cit.) says that he never found it in the "healthy" lice he examined; still, he might well not recognize its presence, if all or nearly all the individuals had been digested, as we have seen may happen in the case of the crithidial forms in the ked. However, I lay no stress upon this point.

Unfortunately, I am not in a position to say anything with certainty as to the customary mode of digestion here. But I feel assured that most important information could be gained by a careful comparison of the course of this process in the normal louse with that in infected lice, and of the character of the "excretory" end-products in the two cases. Nevertheless, certain useful indications are afforded by a consideration of what is already known.

Patton and Craig in their "Text-Book of Medical Entomology" mention that "black particles" are ejected with the fæces. Further, they noted the occurrence, in the lumen of the gut and of its diverticula, of "brownish-black granules, the remains of the digested blood." Therefore, I think it can be regarded as certain that pigment-grains are produced normally as a result of (one variety of) hæmoglobin-digestion. But I am inclined to think that this type of the digestion does not take place *intra-cellularly* in the louse. If a condition occurred resembling that found in mites and the rat-flea (cf. above), I think this would have been observed by Patton and Craig and noted in their reference to the histology of the epithelial cells. Moreover, Rocha-Lima [11], in his note on the normal appearance of the cells in an uninfected louse, does not mention anything of the kind. Neither these authors, nor Wolbach, Todd and Palfrey make any reference to, or show in their figures, the occurrence of pigment-grains in the cells of the mid-gut. This is a most instructive point.

Again, Patton and Craig specially comment on the difference in the cytological character of the digestive epithelium in the louse from that exhibited by those of other insects they examined. For instance, each cell contains from one to three rather small nuclei, which are of markedly karyosomatic type; that is to say, the chromatin is all contained in a conspicuous central body—a chromatin-containing nucleolus. The nuclear structure is thus very different from that shown, for example, by the nuclei of the epithelial cells of the ked, rat-flea, or mite, which are of the usual tissue-cell type. The type of nucleus seen in the louse appears to resemble more that of an egg-cell. Further, Rocha-Lima comments on the intense blue colour of the cytoplasm of normal cells, when stained with Giemsa, another feature in which they recall ova. The point to which I wish to draw attention is that the different cytological character of the epithelial cells in this case may well be an expression of, or indicate a rather different mode of exercise of the "digestive" function from that obtaining in the other cases referred to.

Now, we have seen, on many occasions, that the metabolization of hæmoglobin is *not* necessarily accompanied by pigment-formation (cf. the case of the macrophages, as described by me). Therefore, it by no means follows, from the absence of pigment in the cells, that some form of intracellular digestion of ingested hæmoglobin is not, all the same, normally performed. Both Patton and Craig and Rocha-Lima describe the occurrence of many large or small clear vacuoles, scattered throughout the cytoplasm in normal cells. And I consider that these represent *pale hæmoglobin-"vacuoles"* in an early stage of metabolization. It may be objected that this is impossible because both these authors note the presence of a thick border to the free edge of the epithelial cells—striated, according to Patton and Craig. I reply that this is *not* impossible, because in the case of the epithelial cells of the rat-flea, there is also a distinct, strong, and at times striated border to the cells; this is shown in many of the excellent figures of Minchin and Thomson, e.g., figs. 121, 124, 136, 141 and 142. And many of these particular cells indubitably contain smaller or larger corpuscular fragments or masses of hæmoglobin or pigment-grains, as I have above indicated. How the actual ingestion is accomplished I do not know; but nevertheless it occurs. It is quite manifest that these borders, basement-membranes, etc., to epithelial layers are not so impervious to the passage of "solid" material as has been supposed! Hence, I have very little doubt that the epithelial cells of the louse can and do take in or absorb hæmoglobin, which they metabolize, however, in such a manner that no pigment is produced (cf. the case of *certain* of the epithelial cells of the mite, which also do not appear to form pigment, p. 251, above).

Therefore, I do not think the question of even the intracellular occurrence of pathogenic "Rickettsias" presents any great difficulty of explanation, according to my view. These bodies are to be regarded, I consider, as being produced as the result mainly of an altered, abnormal form of

metabolization of the blood upon which the louse feeds.[1] The various formations appear in the cell-cytoplasm in a manner which is really comparable with the way in which, for example, the intracellular "Rickettsias" of the ked are produced. There is one very interesting observation tending to show, I think, this origin of the "Rickettsias" in the protoplasm itself (in these intracellular cases). Wolbach, Todd and Palfrey (loc. cit.) say that "the cytoplasm of the swollen epithelial cells may be granular and assume forms so like Rickettsia that it is difficult to be certain that organisms are not present." I think this is an illuminating sentence, pregnant with significance! Moreover, a comparison of Rocha-Lima's figures (especially figs. 2 and 3) with his account leads one irresistibly to the conclusion that the more minute forms, the finer "Rickettsia"-granules, are indeed part and parcel of the cytoplasm, that is to say, separately developed in this matrix. The pathological process, in short, seems to resolve itself, largely, into a marked alteration in the character and appearance of the cytoplasm itself. A most illustrative comparison is afforded with the instance I gave above of the appearance, at first, of excessively minute "Rickettsia"-granules in a fragment of cell-cytoplasm in the ked (p. 243).

Rocha-Lima's figures 2 and 3 have puzzled me considerably, when comparing them with his text. His "Haufen" of "Körperchen," which he regards as being cast out of the broken-down cell, are not pure masses of "parasites." They are really cytoplasmic masses and fragments, containing the minute granules; this is manifest from his fig. 3, where the irregular, cytoplasmic mass, broken off from the epithelium, still contains "vacuoles." We have not to do with "einer Entleerung der sich in den Zellen vermehrenden Körperchen"; we have to do with the rupture of the cells and the separation of portions of the cytoplasm containing fine red-staining granules, i.e. *with something extremely like platelet-cytoplasm!* And many of Wolbach, Todd and Palfrey's figures show an extremely similar picture.

While this is a most suggestive point, the enzyme concerned in this metabolization is either not quite identical with, or else it functions in a rather different manner from, that of the macrophages which normally produce platelet granules. Because the formations produced may have, instead, the appearance of rodlets, threads, etc.; and in this respect, the enzyme-action in the louse would seem more comparable with that of the lymphocytes

[1] In the case of extracellular types, like *R. quintana*, the alteration of the hæmoglobin will take place, chiefly in the lumen of the gut. I see that Arkwright, Bacot and Duncan, in a paper on *Rickettsia* and trench-fever (*Parasitol.*, xviii, 1919, p. 92), gave as one of their conclusions that "whether *Rickettsia* constitute the virus of trench-fever, or are in some way produced by it, remains undecided. . . ." It is, I think, only due to the authors to point this out, as showing that, at least at that period, they were not quite certain of the organismal nature of the "Rickettsias."

which can form, at any rate, similar inclusions in the Kurloff bodies.[1] I do not think it is possible to lay too much emphasis on the manifold nature of these cell-enzymes, which can thus metabolize the same substance, hæmoglobin, in so many different ways, both in normal hæmetaboly and in pathological hæmetaboly.

Further, I should not be at all surprised to find that intranuclear "alteration" of the hæmoglobin is responsible for some of the types of form that occur. I mentioned above the occurrence of intranuclear globules of hæmoglobin in the case of epithelial cells of the mite which were becoming effete; and that, in connexion with their attempted digestion, rod-like "Rickettsias" are produced. At any rate, it is clear from the various accounts and figures that the nuclei themselves of the "infected" cells *break down and "disappear."* There, again, "Rickettsia"-bodies of one sort or another will certainly be produced as a result of this karyolysis! The clump of more deeply staining and rather larger "Rickettsias" inside the cell, shown in Rocha-Lima's fig. 2, probably represent the end-products of the nuclear disintegration (no nuclei, as such, being visible).

No, when all is considered, I do not think there is much difficulty in explaining the pathogenic "Rickettsias" on these lines. At any rate, I hope it is clear that it is no use saying that vast masses of minute red-staining granular elements are micro-organisms—whether "filter-passers" or not—without carefully considering the conditions in which they are found and the biology of the associated cells.

Typhus is most probably a Mesodermic Hæmatophagia, caused by a Ferment-Virus.

In my opinion, therefore, my own observations together with my analysis of other work on these Insectan "Rickettsias" strongly support the view I have previously outlined that the diseases with which a few of them are known to be associated are due to pathogenic enzymes. If I may repeat what I said in a recent paper : *they are inevitable witnesses.* This is now generally agreed. Sergent, Foley and Vialatte have recently suggested (*Arch. Inst. Past. Afrique Nord*, September 1, 1921, p. 217) that if the "Rickettsias" do not constitute the virus itself of typhus, they may, nevertheless, be "des microbes 'témoins' qui accompagneraient le véritable agent infectieux invisible." Let us substitute granular elements for microbes and then we have it. "Le véritable agent infectieux invisible!" That, I believe, is the truth; and one which is applicable to all the "Chlamydozoan" diseases, exanthemata, etc. The virus is the invisible pathogenic enzyme,

[1] The observation of Jonesco-Mihaiesti, which I quoted in a preceding paper [20] of the enhanced production of the Kurloff-bodies in a disease which was probably typhus, in guinea-pigs, is significant in this connexion.

attached or "adsorbed" to and carried by the protein particles or bodies of various kinds which are constantly found in such diseases.

Because, if the "Rickettsias" are eliminated as micro-organisms, what other micro-organism can be regarded with any confidence as the cause, for instance, of typhus? There is none. But, in my view, the occurrence of these bodies inevitably implies the presence, also, of some rather modified enzyme, whose action is pathological. *And this is precisely the conclusion at which I had already arrived from a consideration of what is known in regard to the disease itself in man.* This conclusion, in turn, was based upon my discovery—which I regard as being as nearly established as anything founded on microscopical evidence can be—that the characteristic Negri-bodies also represent an unsuccessful attempt on the part of certain nerve-cells to digest ingested red corpuscles.

For details of my argument I must refer those readers who are interested to my first paper [19]; here, I can only summarize the points. The lesions of typhus are essentially in connexion with the endothelial cells—cells of macrophagic type. Hæmatophagy is of pronounced occurrence. In such cells, quantities of "Rickettsias" may be found.[1] These have been regarded as very similar to platelet-granules (Stevenson and Balfour). I have shown that platelet-granules and platelets are formed by cells of this type as a result of the digestion of blood-elements. The virus in the blood *is most intimately associated with the platelets.* Kusama's original work showing this has been confirmed independently by Ségal [14], whose conclusion is that it is associated with these elements and not with the leucocytes. Moreover, the late Mr. Bacot, in collaboration with Ségal [1A] showed, by means of his most brilliant technique, that lice can be experimentally infected by inoculation with platelets, success being evidenced by the subsequent production of the characteristic "Rickettsia"-bodies. Lastly (as I consider) these bodies also are produced in the louse as a result of the abnormal digestion of the blood-elements (mainly the corpuscles) and karyolysis of the injured cells, etc. Now, in the infective platelets where, apart from the platelet-granules, are the "Rickettsias"? Is it not most likely that the latter do, indeed, represent and correspond to the "Rickettsias" in the

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[1] Nicholson (*Journ. Exp. Med.*, xxxvii, February, 1923, p. 224) has subsequently re-stained the same section of a venule, which showed the "Rickettsias" of Rocky Mountain fever after Giemsa-staining, with iron-hæmatoxylin, and found that these elements could then no longer be distinguished; that is to say, he obtained just the same result that I did when examining the granules produced by disintegrating epithelial cells. This indication that the "Rickettsias" do not contain chromatin (or, for that matter, iron, e.g., of the hæmoglobin) is, I think, a fatal objection to the view of their organismal nature. But I fail entirely to understand why Nicholson should consider that this result shows that these bodies and the products of phagocytosis, and of breaking-down nuclei, are different! To me, in the light of my work, it is, on the contrary, one further piece of evidence in favour of the view that they are residual end-products of digestion, etc.

typhus-infected louse? I have above indicated how similar in appearance the smallest "Rickettsia"-granules which have been figured in the louse are to the granules of platelets. And in fig. A (after fig. 7) are shown a few platelets from the sheep, from a preparation kindly given me by Dr. Bedson, in which the platelet-granules are very distinct and conspicuous. When compared with the "Rickettsia"-granules of the neighbouring fig. 7, do not both appear to be essentially the same type of thing? All I can say is that I consider the points I have here mentioned have a very strong cumulative effect. Typhus is a most important instance among the virus-diseases, just because the question of the causal agent can be considered from two sides, i.e., the human and that of the transmitting insect. And regarding it from *both* sides, it is seen that the same conclusion is reached.

The one necessary assumption, so far as I can see, is that the pathogenic enzyme producing this disturbance in the hæmetabolic function of the particular cells concerned can be further produced by the cells themselves on stimulation, as it were, by the minute quantity of the enzyme conveyed by the vehicle of infection—namely, the "Rickettsia"-granules, or the platelets (probably actually the platelet-granules) respectively. Unfortunately this point must for the present remain an assumption. But, as I have explained on more than one occasion, *we have the Twort-d'Herelle phenomenon to guide us.* And with this known instance before us of the further production of a bacteriolysin—i.e., of a non-living substance of enzyme-nature—by living, protoplasmic elements (bacteria) on being brought into contact with a minute amount of this substance, I see no reason to suppose that a similar mode of behaviour on the part of other fundamentally similar living protoplasmic units, namely, tissue-cells, is impossible, or even improbable.[1] Indeed, I think we have a hint, at any rate, in this direction, in the "hormones," a name proposed by W. B. Hardy for the chemical messengers, substances probably themselves of enzyme-nature, certain of which can excite the particular enzyme-activity of various types of secreting cell, quite independently of nerve-control.

If my view is right it will be apparent what enormous diversity, yet extremely minute differences, there must be among these hæmetabolic ferments, produced not only by many types of cell, but by the same type of cell on different occasions. There must be almost as great a variety as there is known to be of toxins and antibodies. Indeed, in Muir and Ritchie's "Manual of Bacteriology," which one like myself not versed in

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[1] I desire to take this opportunity of expressing my regret that I may have—entirely inadvertently—slightly misrepresented Dr. Twort's view in my last paper [20]. I was so pleased to find someone else even admitting the possibility of disease-causation by means of ferment-viruses that I could not forbear quoting Dr. Twort's remarks. It did not strike me that my so doing might lead readers to think that this was the view he held in most favour; I only meant to imply that he recognized its possibility. But I am sorry I did not make it clear that Dr. Twort does actually prefer the view that a living organism of some kind is concerned.

the great science of bacteriology finds most helpful, it is stated (p. 201) that Sidney Martin has suggested that ferments may, perhaps, be looked upon as the primary toxic agents, which act by digesting surrounding material. At any rate, therefore, if my assumption be granted, I do not think it is difficult to look upon certain ferments as potential viruses. In a short biographical notice by Nuttall, of the great natural philosopher, Boyle, it is pointed out (*Parasitol.*, xiii, 1921, p. 407) that nearly two and a half centuries ago he wrote the following words in his essay on the "Pathological Part of Physic": "And let me add that he that thoroughly understands the nature of ferments and fermentation shall probably be much better able than he that ignores them to give a fair account of diverse phenomena of several diseases (as well as fevers and others) which will probably be never properly understood without an insight into the doctrine of fermentation." These words may well show an even greater prophetic insight than Nuttall rightly claims for them.

Of course in most cases the hæmetabolic enzymes or the lytic enzymes of these blood-sucking invertebrates are incapable of stimulating the production of the same enzyme in any type of cell in the vertebrate on which they feed, supposing the latter to become "infected" therewith. Even the enzyme leading to the production of "*R. rocha-limæ*" in the louse, which must certainly be considered pathological to the insect, does not appear to induce any discernible reaction in man. It must, therefore, be in some way very slightly different from that causing typhus. I think it possible that the difference between the two may be manifestly indicated by a slightly different action in relation to the cell-nucleus; if this be so, we have a somewhat comparable difference in the cases of vaccinia and variola where, in the latter only, there is intranuclear hæmatophagy and formation of Guarnieri's bodies inside the nucleus. We know, by an all too sad instance, that infection with the trench-fever virus does not protect against typhus; and this is not surprising because the condition, as manifest in the louse, is very different in the two cases. But it would be most interesting to ascertain whether animals which have been inoculated with the ferment-virus associated with the intracellular "*R. rocha-limæ*"-bodies are rendered resistant to that of typhus.

Again, the enzyme giving rise to "*R. pediculi*" (if this formation occurs independently of trench-fever) must be very closely allied to that producing "*R. quintana*," and, making the necessary modification, I think the view expressed by Hindle [loc. cit.] is quite likely; namely, that under certain conditions, including probably a slightly different physiological state of the type of cell affected in the disease, this particular enzyme in the louse can induce the trench-fever reaction in man.

Although my attempts to reveal the modes of operation of a great principle which has been hitherto unregarded—attempts which, I think, deserve more recognition and support than they have received—are on a

different plane from that of the illustration used by Browning, nevertheless Professor Bayliss expresses admirably in the following words the manner in which such efforts to advance medical science should be prosecuted:—

"*One is tempted to quote Browning:*

'Stake your counter as boldly every whit,
Venture as warily, use the same skill,
Do your best, whether winning or losing it,
If you choose to play!—is my principle.
Let a man contend to the uttermost
For his life's set prize,' . . . .

But at the same time, there must never be the least hesitation in giving up a position, the moment it is shown to be untenable. It is not going too far to say that the greatness of a scientific investigator does not rest on the fact of his never having made a mistake, but rather on his readiness to admit that he has done so, whenever the contrary evidence is cogent enough."

From the Preface to "Principles of General Physiology."

REFERENCES.

[1] ARKWRIGHT, ATKIN and BACOT. "An Hereditary *Rickettsia*-like Parasite of the Bed-bug (*Cimex lectularius*)," *Parasitol.*, xiii, 1921, p. 27.

[1A] BACOT and SÉGAL. "The Infection of Lice (*Pediculus humanus*) with *Rickettsia prowazeki* by the Injection per rectum of the Blood platelets of Typhus-infected Guinea-pigs," etc., *Brit. Journ. Exper. Path.*, iii, June, 1922, p. 125.

[2] BRUMPT. "Au sujet d'un parasite (*Rickettsia prowazeki*) des poux de l'homme, considéré, à tort, comme l'agent causal du typhus exanthématique." *Bull. Soc. Path. Exot.*, xi, 1918, p. 249.

[2A] HINDLE. "Notes on *Rickettsia*," *Parasitol.*, xiii, 1921, p. 152.

[3] HOARE. "Trypanosomiasis in British Sheep" (prel. com.), *Trans. Roy. Soc. Trop. Med.*, xvi, 1922, p. 188.

[4] JUNGMANN. "Untersuchungen über Schaflausrickettsien (*Rickettsia melophagi*, Nöller)," *Deutsche med. Wochenschr.*, xliv, 1918, p. 1346.

[5] LEISHMAN. "Cell-inclusions in the Blood of a Case of Blackwater Fever," JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, xviii, 1912, p. 493.

[5A] LOW and WENYON. "Cell-inclusions in the Leucocytes of Blackwater Fever and other Tropical Diseases," *Journ. Trop. Med.*, xvi, June, 1913.

[6] McCULLOCH. "An Outline of the Morphology and Life-history of *Crithidia leptocoridis*, sp. nov.," Univ. Calif. Publ. Zool., xvi, 1915, p. 1.

[7] MINCHIN and THOMSON. "The Rat-Trypanosome, *Trypanosoma lewisi*, in its Relation to the Rat-flea, *Ceratophyllus fasciatus*," *Quart. Journ. Microsc. Sci.*, lx, 1915, p. 463.

[8] NÖLLER. "Blut- und Insectenflagellatenzüchtung auf Platten," *Arch. Schiffs- und Tropenhyg.*, xxi, 1917, p. 53.

[9] PFEIFFER. "Ueber trypanosomenähnliche Flagellaten im Darm von *Melophagus ovinus*," *Zeitschr. Hyg.*, l, 1905, p. 324.

[10] REICHENOW. "Die Hämococcidien der Eidechsen: 1ste Theil," *Arch. Protistenkunde*, xlii, 1921, p. 179.

[11] ROCHA-LIMA. "Beobachtungen bei Flecktyphusläusen," *Arch. Schiffs- und Tropenhyg.*, xx, 1916, p. 17.

[12] *Idem* (in MUNK and ROCHA-LIMA). "II. Ergebnisse der ätiologischen Untersuchungen des sogen. 'Wolhynischen Fiebers' und deren Beziehungen zur Fleckfieberforschung," *Münch. med. Wochenschr.*, 1917, p. 1422.

[13] ROSENBERG. "Studien über die in- und extracellulär liegenden Rickettsien," *Arch. Schiffs- und Tropenhyg.*, xxvi, April, 1922, p. 112.

[14] SEGAL. "The Association of the Virus of Typhus Fever with the Various Blood-elements," *Brit. Journ. Exper. Path.*, iii, 1922, p. 95.

[15] SIKORA. "Neue Rickettsien bei Vogelläusen (vorl. Mitth.)," *Arch. Schiffs- und Tropenhyg.* xxvi, October, 1922, p. 271.

[16] *Idem.* "Beiträge zur Kenntniss der Rickettsien," *op. cit.*, xxii, 1918, p. 442.

[17] SWINGLE. "On the Life-history of a Flagellate (*Crithidia melophagi*, n. sp.) in the Alimentary Tract of the Sheep Tick (*Melophagus ovinus*)," *Journ. Inf. Diseases*, vi, 1909, p. 98.

[18] WOLBACH, TODD and PALFREY. "The Etiology and Pathology of Typhus," Internat. League of Red Cross Socs., 1922.

[19] WOODCOCK. "An Introduction to the Study of Hæmatophagy," JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, xxxvii, 1921, pp. 321, 418.

[20] *Idem.* "Are the Active Principles of Filter-passing and 'Ultra-microscopic' Viruses Living Organisms or Enzymes?" *op. cit.*, xxxix, 1922, p. 243.

[21] *Idem.* "Hæmatophagy and Hæmetaboly as a Normal Function of Various Types of Tissue-cell," *op. cit.*, xxxviii, p. 403, and xxxix, p. 14, 1922.

EXPLANATION OF FIGURES 15 TO 23.

(All the figures are from Giemsa-stained preparations; and all are of the same magnification, 1,000 diameters. For description, see text.)

FIGS. 15-17.—Epithelial cells of the gut of the ked, showing numbers of intracellular "Rickettsia"-granules; also, stages in the digestion of crithidial parasites. Fig. 15, *a*, a parasite definitely attached to the cell-cytoplasm; *b*, later stage in the incorporation of the body of the parasite. Figs. 16 and 17, *c*, pairs of nuclear elements, the only recognizable remains of parasites in course of digestion.

FIG. 18.—Stages in karyolysis of the nuclei of cast-off epithelial cells of a bird-mite. A, still normal nucleus (perhaps slightly flattened); B and C, early stages in the resolution of the nuclear material into a granular mass; the nucleus in (B) contains two pale hæmoglobin-"vacuoles"; D and E, final stages in the production of "Rickettsia"-granules; F, the granules becoming dispersed. *h*, indicates globules of hæmoglobin of varying size, resulting from early breakdown of the red cells in the lumen (also seen in fig. 23).

FIG. 19.—Unaltered red blood cell of a bird, in the lumen of the gut of the mite, and, just above, a cell in the first stage of alteration. The dark bodies to the right are isolated nuclei.

FIG. 20.—Large epithelial cell (rather flattened-out) containing ingested nuclear material. The cell-nucleus is at the right side; at the left is an ingested epithelial-cell nucleus, undergoing karyolysis, and between the two is the nucleus of (probably) a young epithelial cell.

FIG. 21.—To show epithelial cells of the gut of the mite, containing numerous globules of ingested hæmoglobin; also many pigment-grains, resulting from a previous digestion. The cells may be so packed with the little masses of hæmoglobin that they easily rupture in making the smear. (A, photo taken with a green screen; B, with an orange one.)

FIG. 22.—More epithelial cells, containing ingested nuclear material, which is seen (especially in A) breaking down into granules. No pigment is present in either cell; but in B are several "vacuoles," probably hæmoglobin-vacuoles, in which case the digestion in this type of cell is along rather different lines. Granules are seen in relation with some of these clear areas. In A, there is a small epithelial cell just below the large one, and several squashed nuclei of various kinds around.

FIG. 23.—Nuclear mass in course of disintegration (see text). Many hæmoglobin-"vacuoles" are present, i.e., intranuclear; the majority contain red-staining granules or rodlets (seen liberated at *x*); *h*, globule of hæmoglobin (free); h^1, unaltered intranuclear globule; h^2, much fainter staining intranuclear globule.

FRAGMENTS.

BY COLONEL SIR ROBERT FIRTH, K.B.E., C.B.

XXXII.

As everyone knows, we have had a distinctly unsatisfactory summer; in fact, some people will tell you that we have had no summer in 1922. The result is that the weather has been more than usually a topic of conversation. Man has subdued many things; he has harnessed some of the elemental forces to his use, but in his conflict with Nature he has often done her injury and vulgarized her charms. With all respect to perennial grumblers at our weather, I am tempted to say that fortunately man has not yet succeeded in commanding the seasons, regulating the rainfall, and summoning at will either sunshine or cloud. If ever he attains to this final proficiency, the last province of Nature study which is still reserved to town-dwellers—the weather—will be conquered. Fortunately, I say, the weather has not been interfered with as yet, either as a hobby for man or as a manifestation of Nature. It still remains with us, and can be enjoyed alike from a city window or from a mountain top; for like reason, a rain-gauge can be as useful at Islington as at Oban, and a barometer is as much a barometer in Earl's Court as at Penzance.

Even without such humble instruments of pseudo-science as the rain-gauge and barometer, the weather may be enjoyed, and nobody's weather is so interesting as that of the Britisher, and though the townsman may not be so weatherwise as the countryman, he can at least follow its vagaries. A game of comparison can always be played and plenty of time devoted to a study of the statistics covering a series of years to determine whether the season be normal or abnormal. In this way, mid-January can be made a normal datum line or mean between mid-November and mid-March, and so serve as a sort of half-way house between the ruin of autumn and the hope of spring. Its weather may be sometimes that of the former, sometimes that of the latter. On the same principle, any next twenty-four hours may bring forth as fine a day as one can wish, and that explains why, as I now write, I am not surprised to see June and October meeting during this week in August. Of course, I am not so unreasonable as to expect the victim of a blizzard to think at one leap of the sweltering heat of a hot summer's day, neither do I urge or expect those who bask in the warmth of July to remember the shiverings of December. Our latitude holds forth no such certainty; our weather forbids us who live in it to look far ahead, and if we dislike or do not indulge in long views, we are justified in saying that our weather has taught us otherwise.

The truth is, by taking our weather as it comes, we keep short memories and from forgetfulness we are prone to exaggerate the present. Experience

blunts our memories and shortens our prospects in the matter, so much so that we live in regard to the weather from day to day. Although the average amount of rain and of sunshine has been worked out, there is no telling when and where the next record amount of rain, sunshine, drought, heat or cold may not occur. The uncertainty of these phenomena militates against foresight and the taking of long views; it is only when we are actually up against the emergency that we realize our plight. We are not as others who have learned to discern time and seasons, nor as some who have learned when to sow discord and when to plant the blow. If our political weather experts have been accustomed to watch the steady set of the glass upwards or downwards, they have not always published their warnings, nor if they had it is doubtful if we should have paid attention, simply because our weather-eye has never been adjusted to remote forecasts. There are times when we may perhaps wish it were otherwise, and that our habits and mental outlook were formed not by mere weather, but by a settled climate. But a climate has its drawbacks, physical and psychological; it may involve extremes which are difficult to endure, because an abnormality in a climate is far worse than a spell of untimely weather. Our weather, at the moment, may not be as good as it might be, but we cannot despair of it. We must think of it, at the worst, as only bad weather, such as our forefathers once held through and recognized as bad weather when they spoke of the pilot who weathered the storm. From this point of view, opinions may differ as to whether our weather be a national asset or a national misfortune; but, whichever it may be, it is probably a larger factor in framing our national characteristics than many suppose.

Undoubtedly in its past history the earth has encountered changes in its climates far greater than those which disturb us to-day. One year, it is true, we may be parched by a prolonged and rainless summer, the next subjected to weather that would seem inclement in Iceland; but, on the whole, the average temperature is distributed in zones qualified only by the relation of the land to the milder sea. Although the domination of the sun appears supreme and its orderly largesse of heat the controlling factor in the routine and nature of our seasons, still from geology we know that regions now hostile to life have been repeatedly warm enough to support it in abundance. There have been glacial epochs of intense cold separated by periods of milder climate than that which rules in the same area to-day. Attempts have failed to explain these secular changes in temperature by any astronomical phenomena such as changes in the inclination of the earth's axis, causing greater or lesser reception of solar heat, because there is no correspondence between irregular climatic variations and the rigid sequence of planetary movements. In any event, such factors have not been reconciled with the occurrence of glaciation actually in the tropics, and similar disturbances of the zonal distribution of temperature. The possible explanation is that, although the sun is now

the ruler of our climatic fate, it was formerly assisted and sometimes surpassed by the internal heat of the earth herself. This does not mean that the earth has a molten interior covered by a crust formed by slow cooling, but rather that the influence of radio-active substances within the earth's crust has played a larger part in the production of geological climates than many suppose, and that the radio-active substances have become available from time to time by denudation and by changes of a larger order in terrestrial topography. This is pure hypothesis, but merits consideration if only for the sake of encouraging someone to discover some mode of tapping the radio-active reserves; certainly a radio-active St. Martin's summer would have been very welcome to us in 1922.

XXXIII.

The familiar remark, "A penny for your thoughts," aroused me from a reverie, a few days ago, and I was forced to admit that I had so many things in my mind that I could not easily make a selection and reply honestly, without compromising myself too nakedly. A similar incident must happen to many, and most must admit that, if we are not downright ashamed of a great part of our spontaneous thinking, it is far too intimate, personal, trivial or ignoble to permit us to reveal more than a small part of it. We do not think enough about thinking, and much of our confusion is the result of current illusions in regard to it Our thought moves with such rapidity that it is almost impossible to arrest any specimen of it long enough to have a look at it. During our waking hours we appear to ourselves to be thinking all the time, and some of us are aware that we go on thinking while we are asleep, perhaps even more foolishly than when awake. Unless interrupted by some practical issue, our favourite and spontaneous kind of thinking is what I referred to above as a reverie. In that condition we allow our ideas to take their own course and this course is determined by our hopes and fears, our likes and dislikes. It is amusing and pathetic to observe both in ourselves and others how all thought, that is not more or less laboriously controlled or directed, will circle about the beloved *ego*. We learn generously to overlook this truth, but if we dare to think of it we cannot deny the fact.

I am told that the reverie or free association of ideas is the subject of modern research. I do not know what is the outcome of that research, but there can be no doubt that our reveries form the chief index to our character and constitute a reflection of our nature as modified by hidden and forgotten experience. The reverie is broken only when we have to make practical decisions, and these are the occasions which demand careful pondering and the recollection of pertinent facts. This manner of thinking is more difficult and laborious than the state of reverie; it is the time when we have to make up our minds. Then there is another kind of thinking stimulated when our beliefs and opinions are challenged. Most

of us are curiously heedless in the formation of our beliefs, and few of us take the trouble to study the origin of our cherished convictions; but the moment anyone proposes to rob us of their companionship we are filled with a passion for them which leads us frequently to seek every manner of excuse for clinging to them. The result is that most of our so-called reasoning consists in finding arguments for going on believing as we already do. The real reasons for our beliefs are concealed from ourselves as well as from others. We absorb unconsciously and adopt the ideas presented to us by our environment, and whispered persistently in our ears by the group in which we happen to live. These judgments, being the product of suggestion and not of reasoning, have a quality of certitude and obviousness. On the other hand, opinions which are the result of experience or of honest reasoning lack this quality of primary certitude. This spontaneous support of our preconceptions or reasons to justify our routine beliefs is known to modern psychologists as rationalizing, but actually it is the result of personal preference or prejudice and not of an honest desire to accept or seek new knowledge. In plain words, rationalizing is the self-exculpation which occurs when we feel ourselves or our group to be accused of error or misapprehension.

High and low think in this manner. The reverie and rationalizing go on all the time not only in the mind of the operative or typist, but also in that of the doctor, lawyer, stockbroker, judge and bishop. To affirm this is not to disparage the truly great, but to emphasize the gigantic competition which all really exacting thought has to face, even in the minds of the most highly endowed. Here the perturbing suspicion emerges that perhaps all our vaunted and cherished conceptions and developments in social service, ethics, politics and political economy in the past and present may be swept aside by future generations as mainly rationalizing on our part. Indeed, just as the natural sciences before the seventeenth century were merely rationalizations suited to the sentiments of the period, so the social services of our day may be nothing more than rationalizations of uncritically accepted beliefs and customs.

This may be disturbing, but it brings us to another kind of thought which can be distinguished from the reverie or free association of ideas, from the making up of our minds, and from the self-exculpation called rationalizing. It has not the qualities of the reverie, for it does not circumambulate our personal complacency; it is not made up of the homely decisions forced upon us by daily needs to make a choice of action; it is not the defence of cherished ideas and the finding of plausible excuses for remaining of the same mind; it is more than that, it is that peculiar kind of thought which leads us to change our mind, and that kind of thought which has raised man from primitive ignorance and squalor to knowledge and comfort. This type of thought is called Reason by some, and by others called creative thought because it begets knowledge, and all knowledge is creative. In certain moods, some of us realize that we are

observing things and reflecting with a seeming disregard of our personal preoccupations. In such mood we are not satisfying ourselves with our own complacency, we are not defending ourselves, we are not faced with the need to decide, nor are we apologizing for believing this or that. We are wondering and are the subjects of curiosity and inquisitiveness. For many, this kind of thought is but what may be called idle curiosity, but occasionally curiosity leads to creative thought, alters and broadens our views and the views of others. Some of the greatest advances of modern science have had their origin in simple observations called into life by reflection and curiosity. The cases of Galileo, Newton and Faraday at once come to mind, but there are similar instances in other realms in which the recording and embodiment of acute observations have wrought themselves into the higher life of man. The process by which a fresh and original painting, a piece of sculpture, an oratorio, an opera, a poem or a play have come into being are analogous to that which originates and elaborates so-called scientific discoveries; the only difference is temperamental. Few people trouble themselves about these matters, they take all for granted as they are the voice of the herd, but were it not for the slow, painful and discouraged operation of creative thought through the ages man would never have risen to be what he is. The greater number accept their breakfasts, dinners, trains, telephones, art galleries, orchestras, theatres, their moral code and standard of manners with innocence and disregard, much as a cow asks no questions why it has hay and a dry stall, or a kitten lapping its milk out of a china saucer has no thought about porcelain, or as a dog snoozled up on a sofa worries nothing about, and has no sense of obligation to, the makers of upholstery and down pillows. Few of us are capable of engaging in creative thought, but some of us can at least come to distinguish it from other and inferior kinds of thought, and accord it its proper place as the greatest treasure of the past and the only hope of the future.

These appear to be the various kinds of thinking which we can observe in ourselves and which we have reason to believe go on and always have been going on in the minds of our fellows. They suggest the lesson that we resent criticism of our views exactly as we do of everything else associated with ourselves. Our notions of life and its ideals seem to us to be our own and as such are necessarily true and right. It is clear that our convictions on important matters are not the result of knowledge or critical thought; most of them are prejudices and the whisperings of the voice of the herd. We do not form them ourselves, and we have in the last analysis no responsibility for them and wish to assume none. It is interesting, therefore, to consider the process by which we have reached this condition and how the notions of the herd have been accumulated. If we reflect at all, we recognize that underlying the minds of civilized people there is the animal mind, the child mind, the savage mind and the traditional civilized mind. This means that we are all animals and will remain so, we were all

children at our most impressionable age and can never escape the effects of that; our primitive ancestors lived in savagery and the primitive savage mind is ever with us, and, finally, we are born into an elaborate civilization from whose constant pressure we cannot escape. Each of these underlying minds has its special features; we may grow beyond them and by the help of new knowledge even persuade ourselves that we have successfully transcended them, but, if we are honest, we find that their hold over us is inexorable. The emotions of anger, fear, depression or irritation and the preoccupations of religion, love, war and the chase, stir up impulses that go far back in our history, that repudiate any rationalizing, and prove effectually to us the insecurity of any structure we may have managed to rear on our fourfold foundation. In all our reveries and speculations, we have three unsympathetic and jealous companions: our wild simian forebear, a playful and peevish infant, and a savage. At any moment we may find ourselves seized with an irresistible impulse to show our relation to these old friends, and, what is more, experience an infinite relief in doing so.

If man was and is an animal, why did he alone of all animals become civilized? All animals gain a certain wisdom with age and experience, but the experience of one ape does not profit another. Learning among animals, below man, is individual, not co-operative. Man became civilized because he, alone among animals, had a brain sufficiently elaborate to enable him to form associations and concepts through imitation. Although early man had a human brain he most certainly had not the same ideas and did not make the same judgments as we do. What he saw and heard was not what we should call seeing and hearing. He responded to situations in a kind of impulsive manner. In short, he must have thought much as a wolf does, just as he lived much like him. An owl may look quite as wise as a judge, and to a dog, though he is content to ride in it, a motor-car conveys much the same idea as any common cart, the probable only difference to him being that the one smells of petrol and the other of horses. Only in times of great emotion or excitement can we get a hint in ourselves of our animal impulses free from human sophistication and analysis. The fact is, man started with mere impressions of a general situation, and gradually by his ability to handle things he came upon distinctions which, in time, he made clearer by attaching names to them. We keep repeating this process when we learn about anything. A piano or a typewriting machine is at first a mere mass impression, and only gradually and imperfectly do most of us distinguish the nature and merits of their parts, and realize that there are really many things where only one was first apparent. This process of analysis has been man's supreme accomplishment and that which has made his mind to grow. In such manner the human mind has been built up through the ages by gradual accretions and laborious accumulations. Man started at the cultural zero and had to find out everything for himself, or rather a small number of

inquisitive and restless members of his kind did the work. The mass of humanity has never had anything to do with increase of intelligence except to act as its medium of propagation. Creative intelligence is confined to the few, but the many avail themselves thoughtlessly of the achievements of the more highly endowed. A chimpanzee can fit himself into our civilization and be taught to ride a bicycle and smoke cigarettes, but neither of these things can he understand nor reproduce. At this moment I am writing with a pen and using an electric lamp, but I am incapable of reproducing either luxury or convenience. It is the same with the bulk of mankind.

In like manner history, which is but a name for change or the record of changes, has been due to a small band whose native curiosity outran that of their fellows, and led them to escape here and there from the blindness of their time. Biologically, such men were variations, and their offspring and their fellows had the chance of sharing to some degree the results of their pre-eminence, but otherwise things went on and will go on as before. The nature of the majority of mankind is to be lethargic, easily pledged to routine, timid and suspicious of innovation. Only recently and partially is man progressive. He has spent almost his whole existence as a savage hunter, and from that phase of his mind we can trace back the almost universally accepted idea that man has a soul or double which survives the death of the body. In using this term, savage mind, I refer to the characteristic of the human mind when there was as yet no writing, no organized industry or mechanical arts, no money, no settled life in large communities. We have no chronicles to tell us the story of those long centuries, but we know enough from geological findings to realize that those dim, remote ages must have produced great but inconspicuous originators who laid the foundations of civilization. We have so long taken this achievement for granted that few realize that they ever had to be made at all. Man as a savage may be only a presupposition, but it is a supposition which is forced upon us by compelling conjectural and inferential evidence. At the start man distinguished himself from the group to which he belonged, and when he discovered his own individuality he spontaneously ascribed the same type of individuality and purpose to animals, plants, wind and thunder. Even now we still retain traces of this animistic idea, as when we speak of the spirit of revolt or the spirit of something or other. Later, the dream had a great influence in the building up of man's mind, for it was not merely his shadow and reflection in water that led man to imagine souls, but the visions of the night, and from these beginnings sprang ideas as to religious beliefs. Whole civilizations have been dominated by this savage inference, though it has been revived, rationalized and ennobled by great thinkers from the days of Plato to Christ.

Another elementary factor in man's mental evolution was the tabu or conception of the forbidden thing. Primitive or savage man had tendencies

to fall into habits and establish inhibitions for reasons he either did not discern or easily forgot. These became fixed and sacred to him, and any departure from them filled him with dread. We are not free from it even now; for does not the man who justifies himself by saying that he holds certain views, or acts in a certain manner "on principle," and yet refuses to examine the basis of his principle, rely on psychological conditions similar to those of the savage tabu? When principles and tabus are entrenched firmly in the mind we cannot expect to think freely and fairly, for they stop arguments. It is the same when an issue is declared to be a "moral" one, which is tantamount to implying an emotional state which makes reasonable compromise and adjustment impossible. The great characteristic of the savage mind is its deadly conservatism and its hopeless love of routine. It is to the survival of this underlying element in man's mind that he, like plants and animals, tends to go on from generation to generation living as nearly as may be the life of his forebears. Changes have to be forced upon him by hard experience, and he is ever prone to find excuses for slipping back to older habits akin to his animal and primitive promptings. These are the people whose only advance beyond the savage mood lies in the specious reasons they are able to educe for remaining of the same mind.

Man is by nature credulous. He is victimized by first impressions from which he can escape with difficulty. He resents criticism of accepted and familiar ideas as he resents any disturbance of routine. In this manner, criticism is against Nature, for it conflicts with the smooth working of our more primitive minds, those of the child and savage. Ultimately, in man's history there came men who professed to doubt everything and these were the Greek thinkers who furnished the first instance of intellectual freedom. They discovered scepticism in the higher and proper significance of the word, and by so doing they made the supreme contribution to human thought. Through Socrates, Aristotle, Plato, Democritus, the Stoics, the Eleatics and and the Epicureans we can trace the full stream of scepticism and the metaphysics built upon it. After the Hellenes were absorbed into the vast Roman Empire, critical and creative thought began to decline. New and highly uncritical beliefs and modes of thought became popular. The Stoic and Epicurean dogmas had lost their freshness and men ceased to look for salvation through intelligence and knowledge. Eloquent leaders from the East arose to reveal a new salvation, and Faith ruled in the place of Reason; and the peoples, finding believing to be far easier than thinking, listened gladly to those who said it was necessary only to believe to be saved. This phase marked the dawn of religious and mystical thought which, in contrast to the secular philosophy of the Greeks, dominated the intellectual life of the Middle Ages. For that period the fundamental truths in regard to man were assumed to be established once and for all. The Greek thinkers had had little in the way of authority on which to build, and many of them frankly confessed that they did not believe that such a

thing could exist for the thoroughly sophisticated intelligence. But mediæval thought was grounded wholly on authority. This reliance on authority is a fundamental primitive trait. We have inherited it from long generations of prehistoric men. It is an expression of our spontaneous confidence in everything that comes to us in an unquestioned form. As children we are subject to authority, and later on we unconsciously absorb our ideas and views from the group in which we happen to live. The past exercises an almost irresistible fascination over us. We are tremendously suggestible; our mechanism is much better adapted to credulity than questioning; all of us believe nearly all the time; few doubt, and only now and then.

The findings of modern science have shaken the hold of the sources of so-called revealed authority, but they have done little as yet to loosen our habit of relying on the more insidious authority of current practice and belief. It is painful to most minds to admit that the past does not furnish us with reliable permanent standards of conduct and public policy. We resent that things are not going well and find excuses for turning our backs on disconcerting and puzzling facts. In a word, we are intolerant; but we do not inherit our present disposition to intolerance solely from the Middle Ages. As animals and children and savages, we are naively and unquestionably intolerant. All divergence from the customary is suspicious and repugnant. One of the greatest contrasts between mediæval thinking and modern critical thought lies in the conception of man's relation to the cosmos. To the mediæval thinker, all the heavenly bodies revolved about man's abode as their centre: all creatures were regarded as existing either to assist or to try man. This is a perennially appealing conception of things. It corresponds to primitive and inveterate tendencies in humanity and gratifies, under the guise of humility, our hungering for self-importance. The mediæval and some modern thinkers never question this general anthropocentric and mystical view of the world. To them, all that concerns our deeper needs transcends logic and defies analysis. In order that modern science might develop, it was necessary that a wholly new and opposed set of fundamental convictions be substituted. Man had to cultivate another kind of self-importance and a new and more profound humility. The investigations of natural processes which commenced with the beginning of the seventeenth century opened a new era. The newer methods, slowly elaborated from that day, have resulted in the accumulation of a stupendous mass of information in regard to the material structure and operation of things, and the gradual way in which the earth and all its inhabitants have come into being. Man has come to believe in his capacity to discover important truths through thoughtful examination of things about him, and he recognizes that the world was not made for him but that humanity is but a curious incident in the universe and that its career is but a recent episode in cosmic history. He has acquired a taste for the simplest possible explanation of things, and his mood impels him to reduce everything as far as possible to the commonplace.

This tendency is naturally repugnant to the mystically disposed, but we must come to terms in some way with the emotions underlying mysticism. They are very dear to many, and mere scientific knowledge will never form an adequate substitute for them. We need never fear that the supply of mystery will ever give out; but we must be careful not to see mysteries when there are none and not to fail to see those that we cannot possibly escape. In declaring oneself to be a mystic is not to deny that many things are explicable in scientific terms, any more than in declaring oneself not to be a mystic is to maintain that all things are explicable in scientific terms. Indeed, no thoughtful person will be likely to boast that he can fully explain anything. We have only to scratch the surface of our experiences or the surface of our bodies to find fundamental mystery. And how, indeed, as descendants of an extinct race of primates, with a mind still in the early stages of development can we be yet in the way of reaching ultimate truth at any point?

The changes which are the outcome of modern creative thought have gone farther than merely challenging the validity of authority, as affecting religious beliefs, and the accuracy of old-time ideas concerning the universe. Their effects are manifest in other directions. Means of communication have been so perfected that space has practically been annihilated in respect of speech, and in matters of transport reduced to a fifth. The nations and races of the earth have been brought into such close and rapid intercommunication that they now form economically a loose and as yet scarcely acknowledged federation of man, in which the fate of any member may affect the affairs of all the others, no matter how remote they may be geographically. These conditions have conspired to give commerce and business an overwhelming importance. We no longer make things for the sake of making them, but for money. The chair is not made to sit on, but for profit; the soap is no longer prepared for purposes of cleanliness, but to be sold for profit; the doctor and the lawyer do not give technical advice to their clients for altruistic reasons or motives, but for the sake of fees. Even in literature, be it in the form of books, magazines or newspapers, little catches the eye that is written for its own sake and not for money or business competition. Formerly, the labourer worked because either he could not escape thraldom or because he was a natural artizan; now he is in a position whereby he can combine with others of his kind, lay down terms and conditions of employment, and even enter into business competition with his master. Like his employer, he has learnt to give as little as possible for as much as possible. Similarly, when houses were built to live in and corn and cattle grown to eat, these essential industries took care of themselves; but now that profit is the motive for house-building and grain- or cattle-growing, if the promised returns from those enterprises are less than from making embroidered petticoats or motor-cars, it is not surprising to find that few see compelling reasons for either building houses or raising food. The

printing press has made popular education possible, and popular education has necessitated that all adults should have a vote and exercise some influence in the choice of government and the direction of its policy.

The lesson seems clear that we can learn much from the past in regard to wrong ways of dealing with new ideas. As yet we have only old-fashioned modes of meeting the inevitable changes which loom on the horizon. Repression has now and then enjoyed some success, but, in the main, it has failed. Much will depend on whether our purpose is to keep things as they are or to bring about readjustments. In other words, have we arrived, or are we only just starting? These are questions I leave to the reader to answer for himself, but let him not forget that nearly all men and women, whatever their social and economic status, may and probably have much greater possibilities of activity, thought and emotion than they exhibit in the particular conditions in which they happen to be placed; that in all ranks there is evidence of unrealized capacity; that we are living on a far lower scale of intelligent conduct than is desirable. The manner in which man has revolutionized his environment, habits, conduct and purposes of life by inventions and discoveries, the result of creative thinking, is undoubtedly the most astonishing thing in human history. So recent and rapid has been this change that it becomes increasingly difficult for man's common run of thinking to keep pace with the alterations in his actual practices and conditions of living. Man has never been able to adapt himself very perfectly to his civilization, and there has always been some injustice and mal-adjustment which might conceivably have been decreased by intelligence. With this teaching of the past before us, the existence of great social problems, the prevalence of crude economic theories, discontent with old leaders, standards, criteria, methods or values, and a demand for new ones prompt the thought that unless thinking be raised to a far higher plane than hitherto, some great set-back to civilization may be inevitable. We must endeavour to free our own minds of tabu, prejudice and intolerance, and then do what we can to make others to free theirs. As members of a species that has required a million years to reach its present state of enlightenment, there is little reason to think that any-one of us is likely to cultivate intelligence too assiduously or in harmful excess. If we are to meet and successfully to overcome the dangers ahead, it is clear that we need more mind than ever before.

It is also clear that we can have indefinitely more mind than we already have if we honestly desire it and avail ourselves of resources already at hand. Mind is our conscious knowledge and intelligence, our disposition to increase our information, classify it, criticize it, and apply it. It is obvious that mind is a matter of accumulation not only of mere scientific facts but of facts which concern both the merits and defects, the strength and weakness of humanity—a humanity that has been in the making ever since man took his first step in civilization. From this, I would not have the reader

infer that I underrate the place of science or exact knowledge in the life of man, but the most accurate information available about the world we live in and the nature of ourselves and of our fellow men is not the whole of life, and, except to the few, it can never be the most absorbing or vivid of our emotional satisfactions. We cannot change our natures: we are poetic, artistic, romantic and mystical. We want bread, not a stone; we hunger for the human touch; we resent the cold analysis and reduction of life to the commonplace, and we seek to pursue those things that to many are more enticing than science itself. It is knowledge which has altered our world and we must rely on clear thinking and understanding of our fellows to accommodate ourselves to the new conditions. What we need most is a change of outlook and a chastened mood which will permit an ever-increasing number of people to see things as they are, in the light of what they have been and what they might be. The dogmatic socialist with his unhistorical assumptions of class struggle and his exaggerated interpretations of history sheds no more light on the actual problems than does the obstinate advocate of worn-out shibboleths respecting class and property. We are all purblind, but some are blinder than those who use the various means available for sharpening their eyesight. As an onlooker, it seems to me that the lenses recommended by both socialist and reactionist tend rather to increase than diminish our natural astigmatism.

Never before have we had to rely so completely on ourselves. There is no guardian to think for us, no precedent to follow without question, no lawmaker above, only ordinary men set to deal with perplexities. Man has clamoured for and obtained emancipation; but liberty is a searching challenger, for it takes away the guardianship of the master and the comfort of the priest. Iconoclasm has not freed us; it has thrown us into the water and now we have to swim. Although no previous generation has been so perplexed as ours, yet none has ever been justified in holding higher hopes. Certainly, we must look forward to ever new predicaments, but we may rest assured that nothing is going to be settled in the future in the way things were once settled, for the simple reason that knowledge will continue to increase and will alter the world with which we have to make terms. Life, in short, has become a serious sporting proposition. It rests with us to play the game or refuse to play it. If we elect to play the game we must develop an attitude of mind and high thinking appropriate to the terms and rules according to which life's game must hereafter be played. This means that we have now to make self-denial, love and charity take the place of selfishness, prejudice and intolerance; and as a concise statement of the great revolution we have to face, it means we have now to substitute purpose for tradition as the authority for our thoughts and the manner of our thinking.

THE MEDICAL APPRECIATION OF CAMPAIGNS.

By Major-General S. GUISE MOORES, C.B., C.M.G.. K.H.S.

An appreciation is an expression or statement in writing of the medical requirements of a campaign, considered from an administrative point of view. It should be based on a complete knowledge of the conditions and resources of the country in which the operations are to take place, and should make adequate provision for the proper care, treatment and transportation of the sick and wounded. Besides the medical requirements for the actual theatre of war, an appreciation should include those necessary for overseas communications with the home country, dominions and colonies, direct, or by its intermediate bases.

An appreciation will probably be demanded from the officer selected as the D.M.S. of the army or force; if so, it should rightly be asked for before the campaign is undertaken, and after all available information has been placed at his disposal. It is also conceivable that one might be required from him during its course should any important change of policy be in contemplation involving a considerable alteration in the existing medical arrangements of the force. The withdrawal of the entire Imperial Forces from the Gallipoli Peninsula during the War of 1914-18 is an example of such a change of policy.

The Administrative Medical Officer of a Corps or Division may be required to write a short and concise appreciation on medical situations occurring during the ordinary military operations of his formation or on the occurrence of any outbreak of disease amongst the troops. To enable him to appreciate varied "situations" he requires an intelligent understanding of the lessons which military history teaches; a sound knowledge of war organization and administration, with a correct estimation of their possibilities and their limitations. Added to these, the administrative medical officer should have studied preventive medicine in its application to field armies, and should be either blessed with the gift of imagination, or, so encourage its presence that it will enable him to look ahead and forestall difficulties, or meet them with prompt and accurate decision when they arise.

The recommendations made by the writer of an appreciation must be practicable, i.e. capable of being carried out; and the requirements must always be reasonable and timely in their demand.

To those who contemplate the study of war and its military medical problems, the following campaigns are recommended.

(1) "The Walcheren Expedition, 1808."

(2) "Napoleon's March on, and Retreat from Moscow, 1812."

(3) "The South African Campaign, 1899-1902, with the Commission of Enquiry into its Medical Arrangements."

(4) "The Mesopotamian Campaign, and the Mesopotamian Commission's Report of Enquiry into the Operations of War in Mesopotamia, 1917." Much useful information will also be found in the following books: Larrey (D. J.), "Mémoires de chirurgie," four volumes; McGrigor (Sir James, Bart.), "The Autobiography and Services of Sir J. McGrigor, late D.G., A.M.D."

In expeditions where failure of adequate medical arrangements has been recorded, there were generally in each certain outstanding features of omission which specifically contributed to that failure. Had the medical requirements particular to each campaign been studied, appreciated and provided for beforehand, history could not have blamed the responsible authorities for their shortcomings.

One striking example of such an omission is found in the Mesopotamian venture in the Great War of 1914-18. In that campaign, the absence of any river steamers, equipped for the transport of sick and wounded from front to base, and of any medical establishments for such steamers, had more prejudicial results than any defects of the organization.

It must be remembered that river transport was from the beginning a dominant factor in the medical situation and its sufficiency or insufficiency regulated the movements of the expedition and the evacuation of the sick and wounded. These facts bring into prominence the paramount importance of appreciating the medical requirements of an expeditionary force before it embarks for war.

The following notes may prove useful to those officers whom necessity leads to the study of the subject of this article.

Their perusal will make it obvious, that only the medical requirements peculiar to, and necessary for each particular campaign, need be considered when writing its medical appreciation.

(1) Consider the strength of the expeditionary force and its medical organization. Estimate the probable number of beds required, based on the possession of all information obtainable.

(2) Study the country in which the operations are to take place; its geographical and climatic pecularities; its people; its communications by road, rail and water ways; its general resources as regards housing accommodation, medical material, and comforts for the sick; its civil sanitary administrations; the nature of its soil, and the diseases prevalent amongst its inhabitants.

(3) Consider the variety of the transport that may be required for the sick and wounded, its possible augmentation in the theatre of war, and the provision of extra transport specially suited to evacuation in the war area, such as water-way transport, and in hilly country, pack transport.

(4) Remember that diseases such as enteric, dysentery, cholera, influenza, typhus, &c., follow armies in the field, and be prepared to fore-

stall and combat their prevalance by early recognition, segregation and treament by the use of sanitary methods, and the provision of suitable special hospital accommodation.

(5) Estimate the enemy's strength and resources as regards the numbers and the provision of heavy guns, bombing aeroplanes, trench mortars and gas shells, and realize the possible production of a special class of severe wounds, requiring early treatment and hospital accommodation, possibly nearer than the bases.

(6) When estimating the hospital accommodation remember that long marches in a tropical and sub-tropical climate with insufficient or unsuitable food, and an inadequate or bad water supply, will increase the ratio of sick considerably beyond 3 per 1000 per diem.

(7) The effects of a prolonged and arduous campaign are likely to sap the health and lower the moral of the troops, and efforts should be made by the provision of convalescent depots to counteract such effects, and restore the troops to health and vigour.

(8) Consider the suitability or otherwise of the clothing, underclothing, socks, boots, &c., to the climatic conditions expected at the seat of war; the provision of extra blankets or any other necessary, which will add to the health and comfort of the troops.

(9) Consider all the sanitary precautions and remedial measures (including food), necessary to the maintenance of the health of the troops.

(10) Decide on the policy of sending only severely wounded and medical cases after prolonged illness to the home country, and the retention of the lighter cases in the theatre of operations, or at the intermediate bases.

(11) State your requirements in logical sequence, emphasizing their relative importance, and sum them up at the end of the appreciation.

NOTES ON OUR PRESENT KNOWLEDGE OF SPIROCHÆTAL STOMATITIS, WITH SPECIAL REFERENCE TO THE DISEASE AS IT AFFECTS THE TROOPS.

By Captain D. CLEWER.
Army Dental Corps..

In a communication submitted to the War Office in 1919, and subsequently published in the *British Dental Journal* [1], the writer endeavoured to show that the abnormal incidence of acute fuso-spirillary gingivitis and stomatitis observed during the war of 1914-1918 might well be regarded as the result of a deficiency disease arising from the lack of anti-scorbutic vitamine in the diet of the troops on active service; the gingivitis and stomatitis being due to the invasion by definite micro-organisms of tissues already impaired by a sub-acute form of scurvy.

This theory of the etiology of what, in the light of more recent knowledge, may be better termed spirochætal stomatitis has since received a very considerable measure of support, and it may be well to quote some of the recent opinions in this connection.

Howe, of the Research Department, Harvard University [2], has produced inflammation of the gums and destruction of the alveolar processes in guinea-pigs solely by feeding the animals on a vitamine-free diet. Livingstone, of the Thompson Yates Laboratory, University of Liverpool [3], has cured cases of ulcerative stomatitis by the addition of the necessary vitamines to the diet and considers that the "scurvy theory" has great weight. Handley-Read [4] has had equally successful results by "intensive vitamine treatment," and points out that among Asiatics the teeth drop out in middle age in those districts where the anti-scorbutic factor is deficient in diet, while she also reminds us of the fact that wild animals in captivity develop pyorrhœa alveolaris as a result of abnormal conditions of diet. Jones [5] agrees that there is a greater predisposition to general periodontitis in the absence of a sufficiency of the anti-scorbutic factor, and this has been confirmed by Kritchevski and Seguin [6]. They had the opportunity of observing an epidemic of ulcerative stomatitis affecting more than fifty per cent of a unit in which the men complained of a diet consisting almost exclusively of preserved meat and jams, and they consider that the lesions of this form of stomatitis are akin to those of scurvy. Talbot [7] is also of the opinion that scorbutic conditions are of primary importance in the etiology of gingivitis and pyorrhœa, whilst Swank [8] concludes that "diet is a very vital factor in the maintenance of the body resistance and immunity." *En passant,* it may be noted that the communications of Kritchevski and Seguin, and of Swank, are most comprehensive in their

survey of pathological conditions of the gingivæ and merit the careful attention of those who may be interested in the study of oral infections.

Spirochætal stomatitis, while not so prevalent as it was during the war of 1914-1918, continues seriously to engage the attention of the Army dental officer. The following are some of the reasons for the assumption that the disease is one of considerable military importance.

(1) It is a contagious disease and, unless carefully dealt with, may affect a high percentage of a unit or formation.

(2) A number of attendances is usually essential for the treatment of the condition, periods of training being thereby interfered with.

(3) It is now believed to be the precursor of pyorrhœa alveolaris [6] resulting in loss of teeth and dental inefficiency, and thus necessitating the subsequent provision of dentures with consequent expense to the State.

(4) The dental officer has to dêvote so much of his time to gum treatment that the conservation of teeth cannot be carried out as expeditiously as it should be.

(5) That the disease already causes an appreciable sick-wastage, and unless its incidence can be suitably controlled, it will be a most serious factor in any future campaign and is therefore of paramount interest to the Army Dental Corps.

For these reasons it is proposed to submit a résumé of our present knowledge of the disease and its treatment.

Etiology and Pathology.

Spirochætal stomatitis is now generally believed to be the result of the invasion by definite micro-organisms of tissues already vitiated by some metabolic disturbance. General causes of such disturbance may be found in the specific diseases, intestinal stasis, alcoholism, intoxication by such metallic poisons as mercury, lead, bismuth, etc., and in dietary deficiencies. Local causes of the lowered tissue resistance are brought about by the lack of oral hygiene, the presence of septic roots, impacted wisdom teeth, salivary calculus, abnormal occlusion or the wedging of food. It may be observed at this stage that Talbot [9] has pointed out that the alveolar process is a doubly transitory structure, that it is an end-organ, and that any constitutional disease quickly lowers its vitality. He also observes [7] that sudden changes from heat to cold, or from low to high altitudes, such as are experienced by aviators, interfere very considerably with the metabolism of the body and are liable to produce a degeneration of the alveolar processes favourable to subsequent infection. He suggests that this is a subject for further research and it is one which might be of much interest to those officers of the Army Dental Corps, who are attached to the Royal Air Force.

There can be no doubt but that the infective process is the result of the symbiosis of *Bacillus fusiformis* with certain spirochætæ.

Tunnicliffe in 1911 [10] reached the conclusion that *B. fusiformis* and the spirilla were different forms of the same organism, but modern opinion [6] [11] does not support her view and they are now regarded as being quite distinct. *B. fusiformis* and the various spirochætæ are anaerobic organisms and are grown in pure culture with difficulty.

Noguchi in 1912 [12] obtained a pure culture of a spirochætal mucin-forming organism which he termed the *Treponema mucosum*, and since that date numerous workers have isolated and described various types of spirochætæ. There appear to be three principal varieties; they are termed by the War Office Committee [11] *Spironema buccale*, *S. medium* and *S. dentium*, whilst Kritchevski and Seguin [6] have isolated what are, presumably, the same organisms under the names of *Spirochætæ dentium*, *S. tenuis* und *S. acuta*. *B. fusiformis* is regarded by the last named workers as being a harmless inhabitant of the mouth, although *B. fusiformis* amboceptors have been found in the blood of patients convalescent from Vincent's Angina [11].

So far as the writer knows, it has been found impossible to satisfy Koch's postulates and thus establish any one organism as the casual agent, for when inoculated in pure culture into laboratory animals they have all proved inocuous. Guinea-pigs inoculated with mixed strains of *B. fusiformis* and any one of the spirochætæ have, however, developed localized abscesses from which the causal organisms have been recovered [6], and it would appear that the virulence of the various spirochætæ is greatly increased in the presence of *B. fusiformis*, and that spirochætal stomatitis is the result of a true symbiosis between these organisms. In no case has it been found possible to immunize guinea-pigs and it is equally impossible to prepare sera or vaccines for the treatment of these cases.

It has been stated that the spirochætæ are never found in a clean, healthy mouth [6] but it will be remembered that, out of 3,000 men examined by the War Office Committee, spirochætæ were found in ninety-five per cent of cases [11], whilst Elmerdorf [13] found them present in eighty-five per cent of cases examined in the American Army. The organisms have been found in the clean mouths of healthy children and the writer has always found them present, even in the most healthy mouths, thus suggesting that the statement that they are not observed in a clean mouth can only be regarded as an academic one.

Symptoms.

Local.—The first symptom is a hyperæmia and soreness of the gum margins usually commencing at one of the following points:—

(*a*) The labio-gingival margins of the mandibular incisors.

(*b*) On the pad of gum bucco-distal to the mandibular third molars.

(*c*) The linguo-gingival or labio-gingival margins of the maxillary incisors.

(*d*) The bucco-gingival margins of the maxillary or mandibular molars. The primary infection is more commonly observed in one of the two first-named regions, but any local source of sepsis may determine the location of the lesion.

There is marked hæmorrhage from the gums, which become covered with a typical greyish-white membrane. In a day or so shallow saucer-shaped ulcers appear. There is much tenderness and the ulcers are filled with a greenish-yellow exudate. If untreated, these will coalesce and form a sloughing mass which may extend to any part of the mouth and pharynx. Glandular enlargement is almost invariably present, and cellulitis may supervene.

General.—There is a certain degree of pyrexia, with the normal febrile symptoms, and a prolonged infection is followed by considerable debility and mental depression.

Differential Diagnosis.

From pyorrhœa alveolaris: Pyorrhœa alveolaris is a chronic condition, and should never be confused with the acute manifestations of a spirochætal gingivitis.

From syphilitic or tubercular ulcers: In spirochætal stomatitis there is invariably an infection of the inter-dental gingival processes, and this helps to exclude a diagnosis of secondary syphilitic ulcers or mucous patches, while the ulcers of the condition under review have neither the punched-out appearance of the gumma, nor the undermined edges of the tubercular ulcer.

From diphtheria: The diagnosis from diphtheria is more difficult, as the tonsils and fauces may be involved. The gingival infection is again an invaluable aid, but a bacteriological examination should always be made in any case of doubt.

The organisms of spirochætal stomatitis are easily demonstrated by the use of methylene blue or methyl violet as a stain, but under dark-ground observation these spirochætæ must not be confused with *S. pallida*.

The former are more motile and more loosely coiled than the latter, which does not stain with either of the reagents mentioned.

For a more detailed account of the symptoms and differential diagnosis, the reader is referred to a previous article [1], in which they have been fully described.

Treatment.

Local.—Local cleansing and complete scaling are essentials, but in most cases this cannot be performed immediately on account of the profuse hæmorrhage and tenderness. The number of medicaments advocated for the treatment of this form of stomatitis is legion. Chromic acid, tincture of iodine, hydrogen peroxide, Bowman's solution (liq. arsenicalis and vinum ipecac., with glycerine), sodium perborate, acriflavine,

copper sulphate, mercuric cyanide in 1 per cent solution, dichloramine-T, silver salts and colloids and solutions of salvarsan, being among those most frequently in use. The writer has advocated [1] the local application of copper sulphate in the form of a powder with hydrogen peroxide as a mouth wash, and the former drug has also been used with marked success at U.S.A. General Hospital No. 41, Staten Island, New York [14]. Goadby [15] advised the use of 1 in 200 chromic acid and others have used the pure acid followed by hydrogen peroxide with consequent formation of chromic peroxide. This is markedly efficient in some cases, but is not recommended where extensive ulceration is present, as it appears to act as a powerful escharotic, sealing in a large colony of organisms which flourish anaerobically in the deeper tissues. Neo-salvarsan in various forms has been successfully used in many cases, and recent workers [6] have strongly urged its claims. They maintain that neo-salvarsan is a specific for the various forms of oral spirochætæ and use a 10 per cent emulsion in glycerine locally, aided—in some cases—by intravenous injection of the drug.

It has been proved, however, that mercurial stomatitis is merely a spirochætal stomatitis, originating from the toxic effect of elimination of salts of mercury, via the gum and alveolar tissues [16], and it has been shown by Elmersdorf [13] and the writer [17] that spirochætæ are found in the gums of patients who have had repeated injections of salvarsan and mercury in the treatment of syphilis. In many cases, nevertheless, there is a preponderance of *B. fusiformis* in smears from such patients, while the reverse is observed in cases of simple spirochætal stomatitis.

In a personal communication to the writer on this point, Kritchevski expresses the opinion that, in some cases, the toxic action of the mercury is so severe that the local state of the circulation prevents the penetration of the tissues by the arsenical preparation. Excellent results have been obtained in the treatment by neo-salvarsan of non-specific spirochætal stomatitis and further investigations as to the efficiency of this form of treatment should be carried out.

General.—Castor oil, followed by periodical doses of mist. alba, should be given, and where the pyrexia is severe, a milk diet is called for. Mince diet can be tolerated when the gums are not too sore. Aspirin, salicylates, and the local application of hot boracic fomentations are indicated in cases accompanied by severe pain. Smoking is forbidden, and fresh vegetables, the fresh juice of oranges or lemons, or any kind of food-stuff rich in anti-scorbutic vitamine are of the utmost value.

Prophylactic.—In all units the most rigid oral hygiene should be insisted upon, and the troops should be lectured at regular intervals, as laid down in the "Instructions to Dental Officers." Careful attention should be paid to the diet of the troops, steps being taken to prevent the prolonged cooking of food, and the use of alkalies in the preparation of green vegetables, as both these practices effectively destroy the anti-

scorbutic vitamine. The greatest attention should be given to the careful cleansing of table and cooking utensils, the indiscriminate use of band instruments should be vetoed, and, wherever possible, the isolation of cases of spirochætal stomatitis should be arranged.

It is not clear yet whether there are "carriers" of this disease, but there is every reason to believe that there may be, as men have been observed who presented no clinical signs of stomatitis, although an abnormal number of spirochætæ were found in smears from their gums.

Prognosis.

In simple cases, which come under treatment early, the prognosis is usually good, the condition clearing up in a few days. In neglected cases, or where the infection is severe, it may persist for some weeks, and there is a distinct tendency for the disease to recur.

It must not be thought, however, that this infection is confined to the mouth and pharynx as grave results have occurred when it has manifested itself in other parts of the body. Ellis [18] and Kritchevski and Seguin [19] have recorded cases of death from exhaustion following a severe spirochætal stomatitis, Kline [20] quotes three cases of death from spirochætal pulmonary gangrene, and Bouchut and Leroux [21] a case of spirochætal enteritis with recovery.

To sum up, the need for early and accurate diagnosis, coupled with appropriate treatment, cannot be too strongly insisted upon.

Conclusions.

It would appear that spirochætal stomatitis is the result of the symbiosis of *B. fusiformis* and certain spirochætæ where a soil suitable for their proliferation has already been prepared by the toxic action of some metabolic disturbance.

The factor which is likely to cause most trouble in an Army under active service conditions is undoubtedly the dietetic one, on account of the necessity of issuing rations in which the essential vitamines have been to a great extent destroyed by artificial methods of preservation such as canning at high temperature, and it is suggested that every possible effort should be made to provide the necessary quantity of anti-scorbutic vitamine in the diet of the troops both in peace and war.

The question of the influence of high altitudes as a contributory factor in the case of the Royal Air Force is one which might also be investigated and reported upon, if considered desirable, by those officers who are attached to that Force.

References.

[1] Clewer. "A Contribution to the Study of Fuso-Spirillary Marginal Gingivitis," *British Dental Journal*, xl, 749.

[2] Howe. "The Effect of Scorbutic Diets upon the Teeth," *Dental Cosmos*, lxii, 586.

[3] Livingstone. "Some Acute Oral Diseases," *British Dental Journal*, xli, 508.

[4] HANDLEY-READ. "The Influence of Diet on Dentition," *ibid.*, xlii, 345.
[5] JONES. "Ætiology and Pathology of Chronic General Periodontitis," *ibid.*, xliii, 1.
[6] KRITCHEVSKI and SEGUIN. "Spirochætoses Buccales: Reproduction expérimentale et Traitement," *La Revue de Stomatologie*, 1920, No. 11.
[7] TALBOT. "Interstitial Gingivitis and Pyorrhœa Alveolaris," *Dental Cosmos*, xlii, 856.
[8] SWANK. "Pyorrhœa Alveolaris," *ibid.*, lxiv, 937.
[9] TALBOT. "The Treatment of Interstitial Gingivitis and Pyorrhœa Alveolaris," *ibid.*, May, 1915, per *British Dental Journal*, xxxvi, 618.
[10] TUNNICLIFFE. "Further Studies on Fusiform Bacilli and Spirilla," *Journ. Infect. Dis.*, 1911, 316.
[11] SEMPLE, PRICE-JONES and DIGBY. "A Report for the Pathological Committee of the War Office of an Inquiry into Gingivitis and Vincent's Disease occurring in the Army," JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, xxxiii, 217.
[12] NOGUCHI. "*Treponema mucosum* (New Species) a Mucin-producing Spirochæta from Pyorrhœa Alveolaris; Grown in Pure Culture," *Journ. Exper. Med.*, 1912, xvi, 194.
[13] ELMERDORF. "The Question of Carriers in the Transmission of Vincent's Angina," *The Military Dental Journal* (of the United States Army Dental Corps), iv, 138..
[14] BUEHLER. "Vincent's Stomatitis and Associated Manifestations," *Dental Cosmos*, lxii, 847.
[15] GOADBY. "Ulcerative Stomatitis," A Report to the Medical Research Committee, per *British Dental Journal*, xxvii, 443.
[16] MAUREL. "Mercurial Stomatitis," *British Dental Journal*, xlii, 538.
[17] CLEWER. "Notes on the Treatment of Mercurial Stomatitis," *ibid.*, xlii, 769.
[18] ELLIS. "A Case of Ulcero-membranous Stomatitis (Vincent's Angina) terminating fatally," *ibid.*, xxxviii, 276.
[19] KRITCHEVSKI and SEGUIN. "Fatal Case of Buccal Gangrene following an Extraction," per *Brit. Journ. Dent. Science*, lxv, December, 1922.
[20] KLINE. "Spirochætal Pulmonary Gangrene," *Journ. Amer. Med. Assoc.*, Dec. 10, 1921.
[21] BOUCHUT and LEROUX. "Vincent's Angina and Intestinal Spirillosis," *Progrès médical*, September 18, 1921.

THE STORY OF A BATCH.

By Colonel S. F. CLARK.

With the retirement of Major-General E. G. Browne on June 1, 1922, the curtain was rung down on the batch of February 5, 1887, for he was the last of that company to remain on the stage of the Active List. It has put up a very fair performance during a long run, and it may be worth while to bring together some facts as to what befel it from first to last, the dramas in which it took part, its losses and its rewards, and the places in which its members appeared. This may stimulate young officers of our Corps to follow more closely the career of those who join the Army with them, and it may interest the older men by focusing at short range the corporate work of an average batch.

The "Vision of Mirza" tells of an endless stream of people passing over a great bridge which crosses a wide river, and, as they move along, units of the crowd fall into the water below through holes in the planking—some sooner and some later—until none of those who started together are left. Newcomers take their places, so that those who fall out are not missed in the multitude. This is an allegory of human life, but it can be used to illustrate the passage of a batch of our Corps along the bridge of the Active List, until the holes of retirement, of half pay, or of death, have accounted for all its members.

The batch that this note refers to joined at Netley Hospital on September 30, 1886, forty-one in number, but only thirty-nine were given commissions on February 5, 1887, so that two were unable to overcome the obstacles which studded the approach to the bridge itself.

In the next twenty years only six men dropped out, two of whom failed to last the twelve years which gave field rank to the others. The first to go died of enteric in India at three years' service, four others went down in war, and one retired at eighteen years. Eight then took the pension that became available, but of the twenty-five left only one retired (1911) before age or Royal Warrant claimed him.

When the Great War broke out, nearly twenty-eight years had passed since the batch had first met together, and, as more than half of the men (twenty-one) were still moving, it may be said that so far they had negotiated the dangers of the way very well. Of those missing eight were dead, while the ten who had retired were recalled to service. Those who were still on the Active List had been made lieutenant-colonels in 1911, and in March, 1915, all of them except one, who was on his death-bed, were promoted to be colonels.

As the war went on the members of the batch gradually approached the age limit obstacle, and by Peace Day (June, 1919) only nine were still on the bridge. Three months later all of these had fallen to the four years in

the rank rule, except four who had been promoted to the rank of Major-General. The first of these retired in 1920, the next in 1921, and this year (1922) the last two have dropped through, so that the batch, which was represented on the bridge for nearly thirty-six years, has now been completely engulfed by the river below. As the last man splashed into the water, he had the satisfaction of knowing that he had outlasted every one of the 934 men who were already on the bridge when he stepped on to it, as well as the thirty-eight of his own batch who accompanied him. Twelve of his comrades are now dead, and the survivors are all round about the three score years mark.

The Army List for April, 1887, was the first one in which the batch appeared, and from that time onwards it saw much service in many lands. The pre-war history of its doings shows that up to August, 1914, no fewer than 35 men out of the 39 had served in India, 22 had been to South Africa, and one of these in addition to Mashonaland and another to Matabeleland, and both Egypt and West Africa had seen eight of them. Seven had been stationed in Malta, four in Jamaica and four in Mauritius, while China and Bermuda had each sheltered three. Singapore and the Sudan accounted for two a-piece, while only one man had gravitated to Gibraltar, Ceylon, Crete, and the Egyptian Army respectively. Excluding those who left England for the South African War direct, members of the batch had left home 122 times on tours of foreign service. Each man averaged about three turns abroad, but one of them left England on six occasions, and several went to India every time.

During this period the batch was represented in fifteen recognized military expeditions and campaigns by the undernoted numbers:—

Zhob Valley (1890) 1, Burmah (1891-92) 4, Isazai (1892) 1, Chitral (1895) 3, Ashanti (1895) 2, Sudan (1896) 2, Sudan (1898) 2, Matabeleland and Mashonaland (1896) 2, North-West Frontier, India (1897-98) 6, Lagos Hinterland (1897) 2, Sierra Leone (1898-99) 5, South Africa (1899-1902) 20, China (1900) 2, Mohmand Expedition (1908) 1, and in them gained 61 medals, 2 Ashanti stars, 122 clasps, 3 C.M.G.'s, 3 D.S.O.'s, 1 Osmanieh, 1 Medjideh, 1 promotion to Major, 1 brevet lieutenant-colonelcy, and 16 mentions in dispatches. The price paid was two killed in action in South Africa, and two dead from disease (one of enteric in Ladysmith and one of malaria in Lagos Hinterland), while one was severely wounded in Sierra Leone. In addition two brevet colonelcies were awarded for scientific work before the outbreak of hostilities in 1914—unconnected with any campaign.

It has not been possible to follow the movements of the batch during the Great War, but nineteen of the available twenty appear to have served overseas in various theatres of operations, while the twentieth, in addition to flying visits across the water, was head of arrangements for defence against poison gas—a truly great and responsible position. No records of medals or clasps gained are available, but 12 1914 stars were awarded, and

approximately 9 1915 stars, with 21 General Service and Victory medals. One retired officer served in South Africa. The honours obtained included 1 K.C.B., 2 K.C.M.G.'s, 1 K.B.E., 6 C.B.'s, 8 C.M.G's., 2 C.B.E.'s, 1 O.B.E., 3Legion of Honour (1 commander, 2 officers), 1 Order of St. Sava (Serbia), 2 Order of Avis, both commander (Portugal), 1 Order of the Redeemer (Greece), 1 Star of Roumania (Grand Officer), 2 Croix de Guerre (France), and 49 mentions in dispatches. These "mentions" were spread over 19 different dispatches and included 16 of the batch, ranging from one with 9 appearances to two others with only 1 each, but they were both in the first dispatch of the War.

During its career two of the batch were made K.H.S., one K.H.P., and two V.H.S. One was a temporary brigadier-general and Director of Hygiene at the War Office, another was P.M.O. of the Egyptian Army, one was D.M.S. in Salonica, and another D.M.S. in Mesopotamia, three served on the War Office staff, and one became editor of our Journal.

The record of one officer seems worth noting, as he had his services rewarded eighteen times by his own countrymen, and by at least three of our Allies, thus: C.B., K.C.B., C.M.G., K.C.M.G., D.S.O., K.H.S., V.H.S., one brevet, and ten "mentions," with the Legion of Honour, Star of Roumania, and Order of the Redeemer.

It would appear, then, that the record of the February 1887 batch is one that its members may regard with satisfaction and pride.

Clinical and other Notes.

NOTE ON A CASE OF CHOLELITHIASIS IN WHICH *BACILLUS TYPHOSUS* WAS ISOLATED FROM THE CENTRE OF A GALL-STONE.

By Lieutenant-Colonel H. MARRIAN PERRY.
Professor of Pathology, Royal Army Medical College.

A patient, aged 43, was recently admitted to Queen Alexandra's Military Hospital, London, suffering from symptoms of acute cholelithiasis. The history of the patient was interesting, as he stated that he had suffered from "enteric fever" in South Africa in 1900. An operation undertaken for relief of his condition disclosed a gall-bladder much enlarged; its walls were thickened and fibrosed and within the cavity there were two large gall-stones. The outer surface of one of the gall-stones was seared and the other opened.

In the centre of the stone there was a nucleus of viscid bile, which on culture in suitable media yielded a pure growth of *B. typhosus* giving all the characteristic sugar reactions and agglutinating in specific anti-serum.

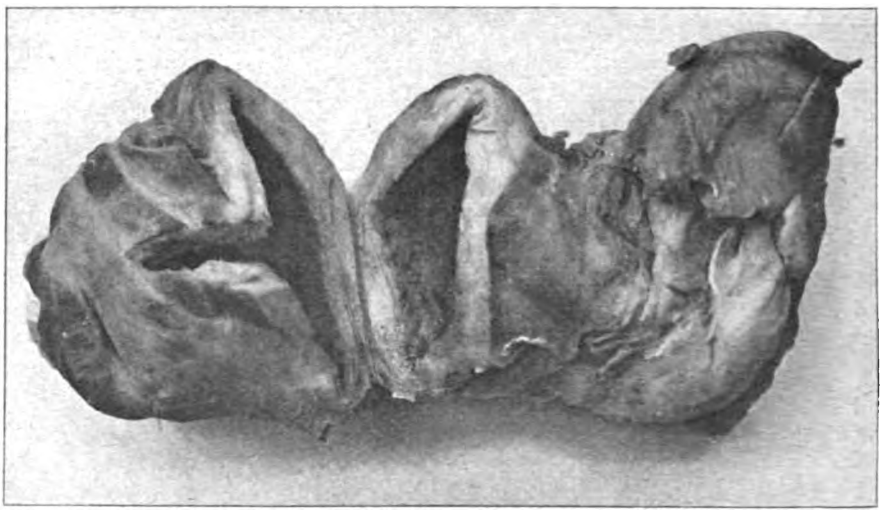
Unfortunately there was no opportunity of bacteriological examination either of the fæces prior to operation, or of the contents of the gall-bladder at the time of its removal, and examination of the stools a week after the operation failed to demonstrate any pathogenic organisms.

Histological examination of the walls of the gall-bladder evidenced the marked fibrosis resulting from chronic inflammation of the viscus. The changes affecting the mucous membrane were more interesting. In places this had undergone atrophy from the pressure of the gall-stones. In other areas the irregular proliferation of the epithelium which had occurred would classify the change as being pre-cancerous.

It is probable that the attack of "enteric fever" twenty-three years ago was a typhoid infection and that catarrhal inflammation of the gall-

bladder induced by infection of the viscus with *B. typhosus* had resulted in the formation of gall-stones. The indirect ætiological relationship of this typhoid cholecystitis to a subsequent possible malignant growth originating in the lining of the viscus is of interest.

The occurrence of infection of the gall-bladder during the course of an attack of typhoid or paratyphoid fever is so common and well known as to call for little comment. The organism reaches this situation in the bile into which it is eliminated from the blood. In the very great majority of cases no evident symptoms are induced by the presence of the bacillus, but in certain instances an acute cholecystitis may result from infection of the viscus. From surgical experience of this condition it has become evident that cholecystitis originating during the course of the disease is not asso-



ciated with the presence of gall-stones, but that the condition of cholelithiasis is encountered in cases which occur at some interval of time, it may be years subsequent to the enteric infection. It must be assumed that in such cases the prolonged infection of the gall-bladder results in a chronic inflammatory condition of the mucous membrane directly exciting the development of gall-stones.

The relation of the gall-bladder infection to the "carrier" condition has of course long been recognized, and the fact that the greater proportion of chronic intestinal carriers have some evidence of chronic cholecystitis has often been placed on record. In the case quoted above no evidence could be obtained of the occurrence of cases of typhoid or paratyphoid infection which could be attributed to the elimination of organisms by the patient.

The illustrations represent the gall-stone from which *B. typhosus* was isolated and the gall-bladder removed at operation.

A CASE OF PSAMMOMA AFFECTING THE SPINAL CORD AND MIDBRAIN.

By Captain A. G. HARSANT.
Royal Army Medical Corps.

Lance-Corporal W., Royal Irish Rifles, was admitted to hospital on December 31, 1920; he had a good family history and no previous history of venereal disease or other serious illness. He complained of a steadily increasing difficulty in keeping up with his platoon on the march (four months): of occasional slight difficulty in commencing micturition (three months): of dimness of vision (one month): and of loss of control over his legs, with unsteadiness on first standing up (two weeks). On admission his general nutrition was good.

He had considerable loss of co-ordination, and a spastic paresis of the lower limbs; marked loss of sensation below the seventh thoracic segment; lateral nystagmus to right and left, and dimness of vision with early optic atrophy.

Gait unsteady and spastic; Romberg's sign marked; legs not wasted; knee-jerks exaggerated; Babinski sign present, right and left; abdominal reflexes present. Loss of painful sensation to pinprick below a line round the waist at the level of the base of the ensiform cartilage; loss to cotton wool and tactile discrimination over an area less extensively by about one to two inches. Marked ataxia and loss of passive position in the lower limbs. There was no zone of hyperæsthesia. Movement, reflexes, and sensation in the arms normal. Pupils normal. Memory and speech normal.

A radiograph of the spinal column did not show any abnormality. Lumbar puncture, without and with an anæsthetic, was dry. Wassermann reaction negative (twice).

For forty-eight hours following the anæsthetic and lumbar puncture, he was depressed, irrational and restless; but gradually returned to his previous contented state of mind.

Three weeks after admission he complained that his vision was suddenly much worse. He could only distinguish light and darkness; all ocular movements were normal. Two hours later he commenced to cry, saying that he could not feel anything: this was followed by loss of consciousness, general rigidity and spasm in the right arm. He regained consciousness in a few minutes but remained very emotional and restless. His legs were now completely paralysed and extremely spastic, any slight stimulation of the foot or leg caused strong flexion of thigh and leg. Abdominal reflexes were lost. Any pinprick above the seventh thoracic segment gave rise to a strong emotional response, accompanied by diffuse protective movements of his arms.

Retention of urine was absolute, and his fundi showed definite optic atrophy.

Four weeks after admission thère was slight paresis of the right side of his face.

After eight weeks.—An attack of loss of consciousness, with general rigidity, followed by dilirium and restlessness for forty-eight hours.

In the ninth week.—A similar attack. There was now some slight astereognosis in the hands. He was quite blind.

Progressive deterioration, emaciation and stupor ended in death six months after admission.

Treatment.—Five intravenous injections of kharsivan led to a merely transient improvement in his mental condition and vesical symptoms.

He was catheterized twice a day for five months after the onset of retention.

Post-mortem examination.—There was pyonephrosis and a small perinephric abscess on both sides.

The pituitary fossa, of normal size and shape, was occupied by an encapsuled growth spreading round the optic chiasma, and on to the inferior aspect of the frontal lobes.

On the posterior aspect of the spinal cord was a growth extending the whole length of the spinal column, springing apparently from the arachnoid mater. The growth was semilunar on section, and compressed but did not invade the cord substance. The growth closely invested the cauda equina, and so compressed the lumbar cord as to render it almost diffluent; but it diminished in thickness in the thoracic and cervical regions.

The meninges around the medulla were thickened, apparently by a tissue similar to that of the growth of the cord.

A section of the growth on the spinal cord was very kindly examined by Lieutenant-Colonel H. Marrian Perry, R.A.M.C., Professor of Pathology, Royal Army Medical College, who submitted the following report:—

"Macroscopic examination of the section showed encapsulation of the spinal cord on its dorsal and lateral aspects by a new formation of tissue.

"Microscopically, this new formation consisted of a somewhat fine connective tissue network which was very vascular and contained many rounded bodies which were concentrically laminated; some of these bodies showed evidence of calcification. In many areas the capillaries, with which the growth was abundantly supplied, had become occluded by proliferated endothelial cells. In its histological structure the growth conformed to a *psammoma*.

"Viewed from the standpoint of an embryological basis this variety of neoplasm is regarded as a lepidoma, i.e., a new growth originating in lepidic or lining tissue. The tissue of origin in the case of the psammomata is the endothelial lining of the smaller capillaries, and, therefore, this form of neoplasm is classified as an endothelial new growth.

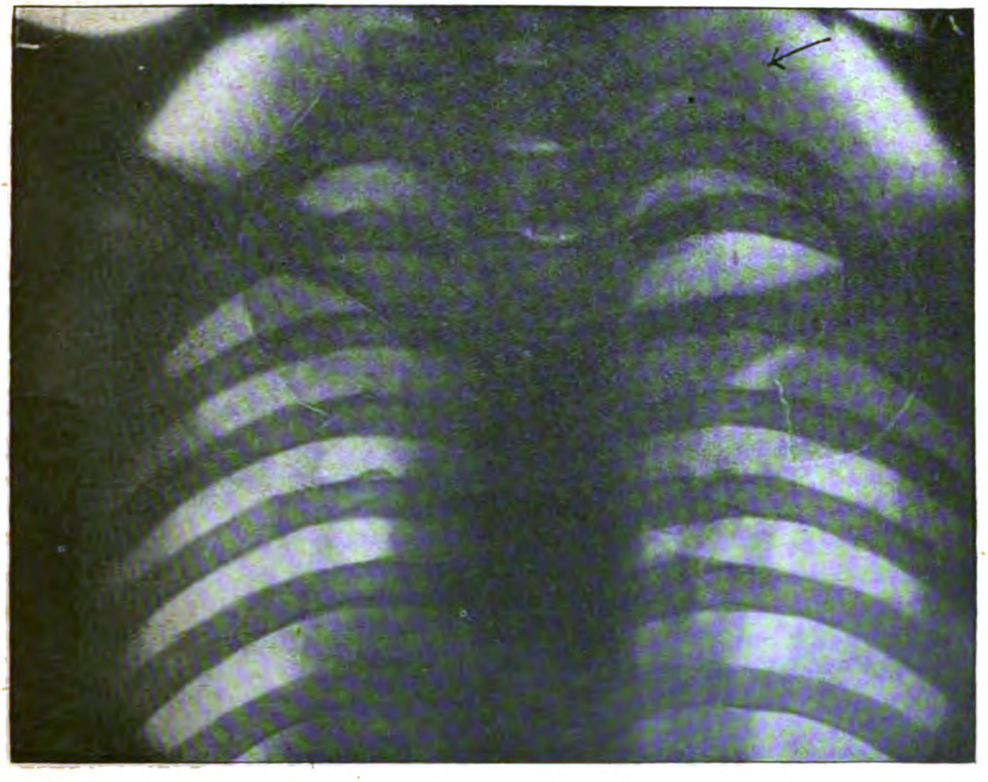
"New growths of this nature, in addition to their occurrence in connexion

with the cerebral and spinal membranes, also may originate from the pleura and peritoneum.

"The rate of growth of these neoplasms is usually slow and, when arising from the spinal membranes, the duration of the case depends on the regional situation of the neoplasm, and the extent to which pressure is produced."

ANOTHER INTERESTING CASE FROM AN OUT-STATION.

By Major J. H. DOUGLAS, O.B.E., M.C.
Royal Army Medical Corps.



X-ray photograph of Pte. H.

On my recall from leave I found attending the morning sick parade, a recruit, Pte. H., suffering from right torticollis.

I treated this case for rheumatic torticollis for a few days, but as it did not clear up with salicylates and iodates, I examined him more thoroughly and found that there was a slight kyphosis in the cervical-dorsal region, with a slight lateral curvature towards the right.

Thinking some tubercular caries might be present causing irritation, etc., of the nerve-supply to the sterno-mastoid, I determined to have the vertebral column in the cervical-dorsal region X-rayed.

A very rare and interesting condition was shown. On the right side the first rib is absent but the second rib has taken on the ordinary characteristics of the first rib except that it is a good deal larger.

The transverse process of the first dorsal vertebra on the right is much larger than on the left but appears to be in one piece, i.e., there is nothing like a rudimentary rib attached to the transverse process.

Editorial.

THE NORTH PERSIAN FORCES MEMORIAL MEDAL.

We have been asked by the War Office to announce the institution of a Memorial Trust Fund, commemorative of the services of the late North Persian Forces, which has been subscribed by officers of the Royal Army Medical Corps and Indian Medical Service who served with these Forces.

In accordance with the desire of the subscribers to encourage the study of Tropical Medicine and Tropical Hygiene it has been decided that the Memorial should take the form of a silver medal, to be known as the "North Persian Forces Memorial Medal," to be awarded annually for the best paper on "Tropical Medicine" or "Tropical Hygiene" published in any journal during the twelve months ending December 31, by any Medical Officer, of under twelve years' service, of the Royal Navy, Royal Army Medical Corps, Royal Air Force, Indian Medical Service, or of the Colonial Medical Service.

The first award will be for the best paper published during the twelve months preceding December 31, 1923.

The award will be announced in the latter part of the year following that in which the paper was published, provided that the Memorial Committee consider the papers published to have attained a standard of merit justifying an award.

List of Officers who subscribed to the Fund.

Captain T. E. B. Beatty, R.A.M.C.
Captain A. C. L. O'S. Bilderbeck, M.B., I.M.S.
Major A. H. Bond, R.A.M.C.
Captain T. C. Bowie, M.B., R.A.M.C.
Lieutenant-Colonel C. H. Bowle-Evans, C.M.G., C.B.E., M.B., I.M.S.
Captain E. Cotter, M.B., I.M.S.
Lieutenant-Colonel and Brevet Colonel H. E. M. Douglas, V.C., C.M.G., D.S.O., M.D., R.A.M.C.

Captain S. Fenwick, M.C., M.B., R.A.M.C.
Captain F. J. Hallinan, M.B., R.A.M.C.
Captain S. J. L. Lindeman, M.C., R.A.M.C.
Captain J. McFadden, M.B., R.A.M.C.
Captain J. W. Malcolm, M.C., M.B., R.A.M.C.
Lieutenant S. C. Mitchell, M.B., I.M.S.
Captain P. G. Russell, R.A.M.C.
Captain J. T. Scrogie, M.B., R.A.M.C.
Captain P. S. Stewart, M.B., R.A.M.C.
Captain C. A. Whitfield, M.B., R.A.M.C.
Captain O. Wilson, M.B., I.M.S.

Echoes of the Past.

A PENSIONER'S EARLY SERVICE.

By Colonel S. F. CLARK.

The writer of these notes served during a period in which great changes took place in the Army Medical Service, and it is thought that a narrative founded upon his military career may be of interest to those coming after, by telling of the work and play and manner of life of the ordinary Army Medical officer for the last thirty years.

A competitive examination for commissions in the Naval, Army, and Indian Medical Services was held in August, 1886. Each candidate had to indicate beforehand which of these services he desired to enter, but all underwent exactly the same tests. After being passed as physically fit, he sat for the written and oral examination in the compulsory subjects—anatomy, medicine, including midwifery, surgery, and chemistry with pharmacy—and was also examined, practically in operative surgery, bandaging, etc., and clinical medicine and surgery. The written papers were set at the University of London, Burlington Gardens, while the oral and clinical work was done at Charing Cross Hospital wards and Medical School. There were also certain voluntary extra subjects, such as French and Natural Sciences, by which a competitor could add to his marks, provided that he had qualified in the compulsory tasks.

It was a curious fact that military tailors got early intimation of the names and addresses of the successful men, and were often the first to carry the good news to them—accompanied by a request for an order for the necessary outfit.

Those who had won places in the Army and in the Indian Medical Service were instructed to report themselves at Netley Hospital on September 30, and to provide themselves with the dress of a surgeon except tunic,

helmet, and sword. The uniforms of these two Services were exactly alike, and were of dark blue, with black velvet facings. The forage cap was encircled by a band of gold lace, over one inch wide, with two narrow black lines in it, while the peak had a wide edging of gold. The patrol jacket, of fine cloth, had a gold false collar, and was fastened in the front by hooks and eyes, right up to the throat. It had no buttons, but was made handsome by being covered in front with broad black mohair braid with lapels hanging down, and with mohair "olives." The trousers had a broad red stripe on the outer side, exactly the same as worn by the Royal Artillery. Wellington boots, white doe-skin gloves, and a "swagger" stick completed this "undress" uniform, but for drill and class work plain coats of blue serge with brass buttons, and fatigue caps were obtained. These caps were made of cloth, and each looked like an inverted boat.

The mess dress comprised a dark-blue shell jacket with a complete edging of gold lace, and facings (collar and cuffs) of black velvet; a red waistcoat, and trousers that had a broad stripe of gold lace carrying two thin black lines on it. The vest hooked right up to the throat—no linen showing at all at the neck—and was ornamented from top to bottom by small brass buttons which touched each other, and forcibly suggested a page boy. One of the front edges of the jacket was embellished in the same manner. This coat was held together by a loop at the throat, but otherwise was worn open, except by the orderly officer, who hooked it right up.

The great-coat was blue with brass buttons, double-breasted, with a detachable blue cape. The buttons had a well-defined edge or border, and the device on them was a seven-pointed star with a small crown at the top in which was a scroll embossed with the words MEDICAL STAFF, enclosing the letters V.R.

The cross-belt and pouch were of black and gold as at present, but the latter had the letters V.R.I. on it. No forage cap or collar badges were worn, but the fatigue cap had the Staff lion and crown badge upon it.

The budding officers duly arrived at Netley, and found that they were known officially as surgeons on probation, or S.O.P's. Most of them were accommodated in quarters, while a surplus overflowed into lodgings in the vicinity, but all had breakfast and dinner at the Mess, while one or two lunched out.

The duties consisted of squad and company drill before breakfast, with attendance in the wards, lectures, and mild laboratory work from 10 a.m. to 2 p.m. Orderly duty from 3 p.m. to 9 a.m. next day was taken in turn.

There were four professors at the Army Medical School at Netley, and they lectured on military surgery, military and tropical medicine, hygiene, and pathology, respectively. They were all old men, three retired officers and one civilian, and eight years afterwards all were dead. The assistant professors, however, were men on the active list.

The course at Netley lasted for four months, and was concluded by an examination in the subjects lectured upon. Failure to pass meant no commission, and the final order of seniority in the batch was determined by the sum of the marks gained in London and at Netley. In medicine and in surgery half the total marks obtainable were allotted to "case books"—things that afterwards became abhorrent to most officers, as they got to be regarded more as a deterrent against ordering any extra food for a patient than as a record of professional interest, because if anything beyond the bare "diet" was ordered for a sick man, his case had to be entered in the book and be kept up. This rule was rescinded some years afterwards, along with the one which compelled the officer of a ward to write *every day*, on each patient's diet sheet, his complete list of extras for that day and his "diet" for the morrow.

As soon as the results of the Netley examination were announced, the Indian Medical Service men, whose commissions dated from the day of their arrival there, went on leave pending embarkation. In those days the service of their Army confrères counted only from the first day of the final examination, and it was usual for them to be transferred direct to Aldershot, but on this occasion the batch were all given leave for one month.

The S.O.P.'s wore one star on each shoulder strap, which denoted lieutenant's rank, as there were no second-lieutenants at that time. Those who had gained commissions were soon gazetted as surgeons, ranking as captains, and became entitled to the two stars of that grade. Their pay was now £200 a year, with quarters, fuel and light, or £90 more in lieu of these. At Netley they had received 8s. a day, from which £5 10s. was taken as a mess subscription. Accommodation was also provided there.

On the appointed day the newly-made officers reported themselves at the Medical Staff Depot at Aldershot, to which they were attached. One of the old wooden huts of Crimean days, in Z Lines, was allotted to the last two men of the batch, while their comrades obtained lodgings in the town. All meals were taken at the Mess, a wooden building in Z—the Medical—Lines, so that residence in the hut had many advantages.

No hospital work at all was done here by the batch, and except for daily attendance at the depot orderly room, and afternoon instruction in a riding school, an open-air life was led on the parade ground, where the men were taught all Medical Corps drills and exercises. At the end of six weeks, examinations in these drills and in riding were held, and certificates of having passed them were given out. Each man was then given two choices as to the military district in which he would like to serve, and, seven months after its arrival at Netley the batch was scattered all over the United Kingdom to begin its real Army life.

The newly-commissioned officers were now in possession of full dress uniform. The "main body" of the tunic was the same as it is now, but the facings were of black velvet, the buttons were of the same pattern as for the great-coat, the piping was red, there were no collar badges, and

the "tail" had only two buttons and no gold lace. The blue helmet is unchanged, and had the Royal Arms badge on it even then. The sword hilt has been made plainer, and the sword knot had black lines running through it. Most of the medical officers on the establishment of Netley Hospital were wearing red tunics and mess jackets, while khaki service dress was quite unknown at home stations in those days.

I was allocated to the South-Eastern District, and on reporting myself at Dover, the Headquarters, was ordered to Shorncliffe for duty. Here I was placed upon the lodging list, and got rooms in Sandgate, away from the camp itself where the garrison was quartered. I did not go near any Mess, and found life rather lonely after hospital hours, so that I began to think there might be better things than being a bachelor. There can be little doubt that this mode of life must have had a good deal to do with the large number of marriages that occurred among junior Army Medical officers.

After two or three weeks I was ordered to Lydd, and marched down from Dover with the 2nd Buffs. A large number of troops was encamped there, and, with the senior medical officer, I lived as an honorary member of the combined R.A. and R.E. Mess. The 3rd and 4th (Militia) Battalions of the Buffs were undergoing their annual training, and at their sports I won the officers' obstacle race, open to the garrison. About the middle of July I returned to Dover with the 2nd Buffs, and was directed to remain there for duty. This was the Jubilee summer, 1887, and owing to the great heat the march back began at the unusual hour, for peace time, of 2 a.m.

I was allotted quarters in the Shaft Barracks, but I had to go to the South Front Barracks for breakfast, as, owing to an oversight, I was not made an honorary member of the Mess of the battalion in the Shaft until some months later. This was one reason why I dined regularly at the Dover Club.

There was a rule in those days that 200 or more healthy soldiers travelling by train must be accompanied by a medical officer—an order which gave me several trips by rail.

My ordinary duties consisted of ward work in the Station Hospital, Western Heights, with medical charge of troops in the Shaft Barracks. This included seeing the morning sick of the unit, and attending to officers and families, as well as looking to the sanitary condition of the barracks. Every Saturday morning all troops were paraded for medical inspection—some years afterwards this became a monthly ceremony instead of being a weekly one.

I was a member of the garrison cricket team in 1887 and 1888, and played some Rugby football in the winter. In December 1887 I obtained sixty-one days' leave, which was repeated in the following July on receiving orders for service in the Bombay Presidency. On my return from this second leave I went on the lodging list, and finally got about one week's notice to embark at Portsmouth.

At that time all movements of troops to and from India were made in the troopships (troopers) "Serapis," "Malabar," "Euphrates," or "Crocodile." They did the Indian run only, as there were other troopers, such as the "Himalaya," for Colonial voyages. They were all veterans, and were withdrawn from service within three or four years of my departure from England in the "Crocodile."

These vessels were built solely for the conveyance of troops, were staffed by officers and men of the Royal Navy, and sailed under the White Ensign. They were large, bulky-looking ships, and took one month to go from England to Bombay. Subalterns were accommodated in what was known as "Pandemonium"—a deck so low down that an open porthole was an unknown thing. Captains did better in the "horse-boxes," a deck higher, while field officers luxuriated in the outside cabins of the same deck, which had portholes well above the water.

There was naval discipline on board these troopers, such as smoking allowed only in certain places and between certain hours; and punishments of defaulters were ordered by the captain of the ship, and not by the men's own officers.

The "Crocodile" sailed about noon on December 3, 1888, and next morning arrived at Queenstown, where the main body of her passengers came on board. It consisted of drafts of various units, so there was no band to brighten things up. The first stop was made at Malta—twenty-four hours—mainly for coaling. The sights on shore included the dead monks, who cannot now be seen. The visitor was shown into a bare building with large niches in the walls, each containing the embalmed body of a monk in an upright position, supported by a wooden bar across the mouth of the cavity. It was a weird sight to see those long-dead men standing there—dead, but not buried.

Shore leave was given at Port Said and also at Suez, while on the night of January 1, 1889, the Colaba light was seen—the first glimpse of India for most of those on board; and next morning the ship anchored in Bombay harbour. The troops were taken ashore in lighters to the Sassoon Dock, where accommodation for all ranks and their families existed, and in the evening all who were for Bengal and Bombay left in troop trains for Deolali, which was reached next morning. In this well-known place they were allotted quarters in rows of buildings, and at the mess for officers the griffins first heard the historic call, "Qui hai," from old hands, who impressed upon the perspiring new-comers that a cool snap in the cold weather was on.

Each evening contingents of "Crocodiles" left by train for up-country—each officer accompanied by the native butler he had engaged from the mob which presented itself at Bombay. At Deolali I found that my selection was a known wrong 'un, so he was replaced by a local native who served me well. About January 7 I left for Khandwa Rest Camp, arrrived there next morning, and stayed for two days in the quarters provided.

One more night in the train brought me to my destination, Mhow, in Central India, where all hands as usual went to the Rest Camp. In the afternoon I moved into the Dâk Bungalow, or Travellers' Rest House, and in the evening went down to the railway siding to see the remnant of the "Crocodiles" off on their journey to Bengal. When the red tail-light of the troop train disappeared round a curve, I felt somewhat a lonely stranger in a strange land.

Next morning I reported myself for duty at the Station Hospital, and later in the day moved into a bungalow occupied by a young Indian Medical Service officer who was temporarily attached to the hospital for duty. For a few days we both had our meals at a British infantry battalion Mess, but then we formed a "chummery" with two other medical officers, as four of us seemed too many to trespass upon the hospitality of another unit. As the bachelor officers gradually left the station and were replaced by married men, the chummery slowly dwindled away, and finally ceased to exist. During the whole of my service in Mhow I remained in this bungalow, and except for the first few weeks was its sole occupant. I never made use of my honorary membership of any Officers' Mess.

My work consisted of duty at the station or auxiliary hospital—before breakfast—with medical charge of the artillery, cavalry or infantry districts, all of which I held in turn. This included the same duties as in England, and necessitated the use of a horse, for which no allowance was made in either money or kind.

In 1890 nursing sisters were introduced into military hospitals in India. This was a great step forward, but there were still no trained male attendants, and a severe case was nursed by men of his own unit, who were innocent of any knowledge of the subject until they picked up something as they went along. The assistant surgeons—or apothecaries as they were called then—and native ward boys, constituted the permanent nursing staff. The medical officer was expected to deal with any kind of case, or with anything that came along. Specialization was unknown, and a laboratory was unheard of, and as one looks back one can see what a large margin existed for the reduction of sickness and of mortality in India.

When I arrived in that country many things appertaining to disease that are now common knowledge were unknown. The connexion between mosquitoes and malaria had not yet been given to the world, and the only things really known about this malady were that it usually yielded to quinine, and that it flourished in marshes and such like places. The protection given by mosquito curtains was noted, but was supposed to be due to dampness in the air being kept off the sleeper.

Enteric fever was so rife that reams were written on its clinical aspects, and about methods of treatment, but bacteriological knowledge of it was scanty. It was not considered to be infectious, but one heard stories of attendants on a case contracting the disease, and suspicions that a bed-pan

had carried it from one patient to another, so that there was a general feeling that this idea was incorrect. The fact is that very little was really known about enteric fever, and opportunities of research were practically out of reach of the ordinary medical officer.

Cholera had not yet been stamped out, but it no longer swept men away in scores. It may be said that in those days the medical services were only groping towards the scientific basis on which present-day knowledge of tropical diseases has been built up.

Troops changing station usually went by road, and I spent many weeks marching with cavalry and artillery. I also went to several artillery practice camps, and was held in readiness on several occasions to proceed to cholera camps.

The official telegraphic code words relating to the progress of affairs during an outbreak of cholera, were a collection of unpleasant medical terms, such as bile, blood, fæces, etc., that were softened down some years afterwards. They were recorded in Volume VI (Medical) "Indian Regulations," a large red-covered book which every medical officer was supposed to possess. It was hard to get, and at the half-yearly inspections at which books were shown, many ingenious devices were used to cover the absence of this work. It was rumoured that an Army and Navy Stores price list had been successfully used as camouflage in this respect.

The belief was current that any money-saving device which was unknown to the Indian Government was not worth knowing, and the comparative cheapness of junior medical officers was regarded as the reason why India was the first foreign service station of the vast majority of these officers. The *British Medical Journal* once remarked on the unfairness of unloading such a large proportion of comparatively inexperienced practitioners upon the British troops in India. A medical officer under five years' service was rewarded with 317 rupees 8 annas per month. On completion of six years he got 433 rupees, with some annas and three pi, and to accustom him to this dazzling wealth, he got 18 annas extra per month for the one year between. In those days the rupee touched its lowest record value, so the young medical officer who had been getting £200 per annum as pay in England, with lodging, fuel and light, and servant allowances amounting to another £90, found himself drawing about £190 per annum in India, out of which he had to pay house rent, servants, light, and everything else. There were even no such things as travelling and field allowances, while each officer had to buy himself a tent, instead of being paid for living in a public one, as at home.

The Indian Government seemed to be able to ignore the "Warrant" under which medical officers in the other parts of the Empire served, and the manner in which the junior officers were underpaid was little short of scandalous. Not only was horse allowance refused, but I was told that it was under a comparatively recent order that a mounted unit had to supply a charger, properly equipped, for the medical officer who accom-

panied it on a march or other duty. This instruction was said to have been issued in consequence of the plucky conduct of a medical officer, who was detailed to proceed with a battery of artillery on a march of several weeks' duration. He refused to mount himself as no horse allowance was payable, so as the battery gave him no help, he started with it on foot, and when it got ahead out of sight, he walked home again, and said that he had lost the battery as it had gone right away from him.

In my Indian service elephants were borne on the strength of the forces. One of the units at Mhow was a siege battery, armed with forty-pounder guns drawn by these animals. As each gun had a tandem team of two elephants, and each ammunition wagon was pulled by twelve bullocks, two abreast, the battery in column of route was an interesting sight. Elephants were also used by the transport service, and the story may be recalled of the exasperated "Tommy" who was unable to prevent his load from constantly falling off one of these animals. He looked at its trunk and then at its tail, and addressed it to the effect that if he knew which end was its face he would kick its stern.

There were ample means of outdoor recreation at Mhow, and I took my share of everything except polo. At cricket I was held to be the best bowler among the officers, and when my side was batting the opposition was always pleased when my wicket fell. I also sang in the church choir.

Blue uniform was used in the cold weather only, while during the rest of the year khaki, or white, drill was worn. Medical officers had black gorget patches on the front of the collar of the khaki coat, with a brass letter M on the shoulder straps. Later on the M was displaced by A.M.S., and finally R.A.M.C. was worn. About this time sabretaches were done away with for all arms, including mounted medical officers, and field officers had no longer to provide themselves with *brass* spurs and scabbards.

(*To be continued.*)

Current Literature.

Morphologische Studien an Influenzabacillen und das ætiologische Grippeproblem. Levinthal and Fernbach. *Zeitschr. f. Hyg.*, 96, 1922. Pp. 456-519.—The author's studies have proceeded upon the basis of the following two theses: (1) Influenza is an infectious disease of periodic character, occurring in all temperate climates, which only acquires a wide epidemic, or even pandemic distribution at intervals of decades; and (2) the most important result of all the international investigation since 1918 is the knowledge of the regular occurrence of Pfeiffer's bacillus in the disease. The question to be decided is whether the recognition of this fact is sufficient to explain the ætiology and epidemiology, or whether the addition of some other factor is necessary.

(i) *Observations on the Occurrence of Influenza Bacilli in Cases of Influenza during the years* 1918 *to* 1922.—The bacteriological results of the first period of the pandemic showed two points: examination of the sputum alone without the assistance of other complementary methods, such as the "Hustenplatte," or swabs from the posterior naso-pharyngeal cavity, gave positive results, in clinically and epidemiologically typical cases, in fifty-nine to seventy-six per cent; examination of more general, unselected material, or of suspicious, sporadic cases at once lowered this percentage from thirty-five to twenty-one, an indication how closely the occurrence of the causal agent, in this commencing period of the epidemic, was associated with the path (or track) of the disease, and could not by any means be looked upon as ubiquitous.

The earlier observations, by Levinthal, of the occurrence of agglutinating antibodies in the serum of influenza patients, for selected antigenic material, were confirmed in the summer of 1918, when a large increase in positive results was obtained. It was noteworthy that among twenty-two serologically negative cases, nine (forty-one per cent) were bacteriologically positive; this observation tells against the view, which has been expressed as being against the significance of the Widal in a positive case, that the mere presence of the bacilli, irrespective of their pathogenic rôle, would suffice to give a serological reaction. On the other hand, the serological diagnosis was made in seven cases which remained bacteriologically negative. In an early period of an epidemic, therefore, the value of the Widal must be recognized, whereas manifestly, to-day, after the drastic searching through which every country has passed, its value is considerably restricted.

In the later spasms of the epidemic, work with the sputa was more and more supplemented with the use of the "Hustenplatte" (i.e., the "Hustenaussaatmethode" of Chievitz and Meyer, introduced by them for whooping-cough) and with swabs from the hinder naso-pharyngeal cavity.

Numerous tables of the bacteriological findings from such material and from various organs at post-mortems are then given. The upshot of the authors' experience during the whole period was that an enormous increase in number of the influenza bacilli occurred in the sputa examined, and they consider that they show conclusively the regular prevalence of Pfeiffer's bacillus in the various material studied throughout the whole period of the pandemic. They think their success was largely due to the use of optimal culture media.

(ii) *Culture Media used.*—Before considering the superiority of modern special media, the authors give details about the preparation of the customary mixed blood-agar plate, because this is indispensable for the differentiation of the various types of the influenza bacillus, especially for their fourth type, the hæmolytic one.

The preparation, advantages and disadvantages of three special media are then discussed, namely, Levinthal's steamed blood-agar, Avery's oleate-hæmoglobin agar and the chocolate blood-agar of the Americans. An

advantage of the steamed blood-agar lies in the prolific growth of influenza bacilli which is induced, especially relatively to that of other organisms. But the authors praise highly the oleate-hæmoglobin agar for its remarkably elective action, which thus facilitates enormously the detection of the bacilli; for this reason it is the choice *par excellence* for the primary culture, especially in the case of workers of less experience. To this point, however, they consider the advantage of this medium is practically limited. Where a complete picture of the bacterial flora is desired in its quantitative relations of predominating and accompanying forms, the superiority lies with the other method. Certain other slight disadvantages (e.g., its poor keeping quality) of the oleate-hæmoglobin agar as compared with the steamed blood-agar are referred to. As regards the chocolate blood-agar medium, the authors consider that its non-transparent character is a considerable disadvantage.

The authors believe that the secret of the value of the steamed blood-agar is to be explained by the heating resulting in the splitting-off from the red corpuscles of the substance X (present in hæmoglobin) and its optimum solution in the medium. Continued or over-heating, on the other hand, destroys the vitamine-like substance, V. Hence this is to be avoided, as both these substances have been shown to be necessary for the growth of these hæmophilic bacilli (Davis, and Thjötta and Avery).

(iii) *The Occurrence of Influenza Bacilli in Non-influenzal Cases from* 1919 *to* 1922.—This section deals with the occurrence of influenza bacilli among healthy people and in cases of various diseases, namely, measles, whooping-cough, scarlet fever, diphtheria, tuberculosis, during the above period. Tables of the findings of the four different types are given. A point noted is that, if the positive findings of types III and IV, for the year 1921, from healthy people and from cases of diphtheria, are added to those of types I and II from the same sources, the whole percentage is raised from thirty-two to seventy-one, and from forty-three to sixty-two, respectively.

Although only a few examples are discussed, the authors lay stress on the variation in type of the organisms, in relation to different conditions as regards health, of particular cases at different periods. This variation is compared with that of diphtheria bacilli, their change into diphtheroids, and the further reversion at times to the pathogenic type. The hæmolytic type is considered to be the characteristic site-variety of the throat. The large number of healthy carriers on the one hand, and the regular occurrence of the bacillus at the site of the disease in cases of epidemic influenza, on the other hand, is indicative of the varying virulence of Pfeiffer's bacillus.

(iv) *The Four Morphological Types of Pfeiffer's Bacillus.*—The authors consider that the serological and biological behaviour of the influenza bacillus indicates rather an extremely labile nature than a heterogeneity. And they have had in view the possibility that such plasticity may be

correlated with a corresponding fluctuating virulence, and therewith afford a basis for epidemiological conclusions.

The four types recognized are thus distinguished: (1) the true influenza bacillus of Pfeiffer; (2) the pseudo-influenza bacillus; (3) the extreme pseudo-form; and (4) the hæmolytic type (*Bacillus X*).

Type I is regarded as the basic or primary form of the organism. Both on optimal media and on the usual blood-agar plate, it appears as minute, short rods of great uniformity, comparable with the appearance of a *melitensis* culture.

Type II, on the other hand, shows considerable pleomorphism. On the steamed blood-agar the rodlets predominate, but these already tend to be elongated and more pronouncedly rod-like; certain individuals, indeed, grow into narrow, thread-like forms. On the customary blood-agar plate, these thread-like forms are more abundant and the pleomorphism more marked, giving the characteristic appearance of the pseudo-type.

In Type III this pleomorphism assumes grotesque forms, justifying the designation of extreme pseudo-type. All transitions can be met with from minute coccoid forms to fantastically thick knots and coils. Here, too, this pleomorphism is more pronounced on the ordinary medium than on the optimal media. The microscopical appearance itself gives the impression that degenerative processes are concerned; an impression heightened by the increased fugitive character of this cultural type, which either fails on sub-culture or else reverts to Type I.

The appearance and behaviour of Types I—III on solid media are macroscopically the same. But even on steamed blood-agar, at least in the primary culture, the colonies of Type IV can be distinguished after practice. The large colonies are distinctly more opaque and more thickly formed; though on further cultivation these are soon replaced by the clear, structureless growths of the true influenza type (I). On the mixed blood-agar plate, however, there is a marked distinction. Here all fresh cultures show intense hæmolysis, which, as in the case of strongly hæmolytic streptococci, leads to the complete breakdown of the blood-pigment, and the entire clarification of the medium. Rabbit-blood proved superior to horse-blood for the observation of this phenomenon. Different strains behave differently on sub-cultivation in the course of months. In one case no diminution in the production of this hæmolysin has so far been observed; but in other cases, after about three months, the hæmolytic property had become almost completely lost, although the same strains in the throat of their original providers have retained this property over a much longer period.

The microscopic picture of this Type IV, first isolated by Pritchett and Stillmann from pharyngeal secretion under the name of *Bacillus X*, resembles that of the extreme pseudo-type (III), with very varied formation of stout threads and massive rounded forms in different strains. The basal form of this type appears to be a stout, straight or slightly curved bacillus, which not infrequently shows a little conidial-like bud at either or both ends. Various other points are mentioned.

Common to all four types is the mode of growth in bouillon, limited to the ground sediment, without producing any cloudiness of the superjacent liquid.

Observation of the various types over an extended period shows the close connexion of Types I—III. On optimal media, the transformation, both of the pseudo type and of the extreme pseudo type, into Type I can be effected without trouble.

The authors regard equally the hæmolytic type as a modification of Pfeiffer's cocco-bacillus, under the influence of the infected organism. They recognize, however, that the question of the intimate relationship of this type is more difficult, because the involution proceeds much farther, beyond the boundaries circumscribing the other three. As regards the most characteristic feature, the production of a hæmolysin, they found that this disappears, at least in some strains, on long-continued cultivation. But they did not succeed in bringing about the reversion to the true primary form of the bacillus.

Another difference from the non-hæmolytic types is revealed by testing the hæmoglobinophily. Although all the X-strains isolated would no more grow on the ordinary agar-plate than would the true influenza bacilli, yet the addition of thirty per cent ascitic agar permitted a minimal growth to be recognized. And in symbiosis with yellow staphylococci, the same slight growth could be obtained in the neighbourhood of the satellite bacteria, even on ordinary agar. Whereas their oldest influenza strains, on the other hand, show absolutely no growth on ascites plates inoculated with the staphylococci. The question is raised whether under certain conditions Pfeiffer's bacillus in the human organism may not be transformed into an entirely saprophytic, no longer hæmophilic type, which can grow on ordinary agar. In this connexion reference is made to a strain of this kind, isolated by Neufeld and Papamarku.

Endeavours were made to determine the relation of this *Bacillus X* to the other types by means of cross-agglutination experiments. The authors consider that, so far as any conclusions can be drawn from serological investigations on Pfeiffer's bacillus, the inclusion of all four types in one species is clearly justified.

On the other hand, all absorption tests were most unsatisfactory in regard to both groups, true and hæmolytic.

Further evidence pointing to the specific unity of the influenza-rods and the "X" bacilli was obtained on the lines of Davis's work on the satellite (or symbiotic) phenomenon as an aid to the classification of hæmophilic bacteria. Influenza bacilli of Types I and II, on the one hand, and Type X on the other, were reciprocally tested on horse-blood agar, and it was established: (1) that all these races or strains show equally, with yellow staphylococci as nurse bacteria, the satellite phenomenon; and (2) that the production of giant colonies could not be induced in influenza strains through the medium of the X-strain, and conversely, neither in the latter through the medium of the former.

The conclusion is reached, therefore, that in addition to the true coccoid influenza rodlets, three other morphological types of influenza bacilli, the pseudo-form, the extreme pseudo-form, and the hæmolytic X-form are variants of one true species. Types II and III are regarded as labile varieties, while Type IV may represent a persistent modification.

(v) *Epidemiological-ætiological Conclusions.*—In spite of the recognition of the fact that Pfeiffer's bacillus is the most regularly occurring organism in cases of epidemic influenza, the authors are in complete agreement with the critics of Pfeiffer's doctrine who consider that the bacillus, without some additional factor, cannot be regarded as the primary agent of the disease. After a discussion of recent work on the subject, including a consideration of the filter-passer of Olitsky and Gates, the authors conclude in favour of the view that Pfeiffer's bacillus is the ætiological agent of influenza, through the acquirement at times of a greatly increased virulence, which subsequently is again lost. And they consider that this labile character of the virulence has its morphological counterpart in the varieties of type which they have found.

Pasteurization of Milk. By Joseph Race. *Chemistry and Industry*, vol. xlii, No. 8, 1923, p. 166. Pasteurization at 145° F. for thirty minutes, does not cause in milk any appreciable change chemically. The soluble phosphates of calcium and magnesium are not rendered insoluble, the albumin is unchanged, but the titrateable acidity is slightly diminished due to loss of CO_2. If the temperature is allowed to rise to 150° F., part of the albumin is coagulated, and the cream is diminished in volume.

The taste is not altered at 145° F., nor are the enzymes destroyed. As regards vitamins, very little work has been published in this connexion, but the Ministry of Health are conducting experiments. Data so far available show that there is no effect on the vitamins.

Bacteriologically, pasteurization at 145° F. reduces the bacterial content of milk and, if the protection is not absolute, the method is still considered satisfactory from this point of view. Emphasis is laid on the extreme care necessary to ensure that no part of the milk becomes heated over 145° F.; in efficient pasteurization recording thermometers must be used.

Ship Fumigation. *Public Health Reports of the U.S. Public Health Service*, vol. xxxvii, No. 44. This preliminary report recapitulates the disadvantages of sulphur dioxide as a fumigant and the advantages of hydrocyanic acid gas, noting that a number of fatalities have occurred with the latter gas, owing to its being odourless and non-irritating even in toxic concentration.

The report then discusses experiments with a view to using hydrocyanic acid gas to which has been added a percentage of an easily detectable irritant gas.

Good experimental results were obtained with a mixture of hydrocyanic

acid gas and cyanogen chloride generated by: 4 ounces powdered sodium cyanide; 3 ounces sodium chlorate; 2 ounces talc., (to reduce fire and explosion risks); 17 fluid ounces commercial hydrochloric acid (sp. gr. 1·20); 34 ounces water.

The talc. is mixed with the sodium chlorate, then the cyanide is added and mixed, and then a bag containing the mixture is dropped into the diluted acid.

Using the above quantities for 1,000 cubic feet, the investigators note that the resulting cyanogen chloride and hydrocyanic acid gas mixture was found to possess high toxicity with rodents, bats, cockroaches, and bed bugs (the concentration for lice destruction requires further investigation), and to have no effect upon foods, tobacco, leather, and no corrosive action on metals (with the possible exception of nickel). It possessed satisfactory penetration qualities and was not more persistent than hydrocyanic acid gas alone.

The outstanding advantage of the mixture, as compared with simple cyanide gas, is its easy detectability. In a one-eighth lethal concentration (having no harmful effects on rats after one hour) lachrymation on exposure was still extreme and the risk of accidental fatalities thus becomes negligible.

A fuller report is promised later.

Bacillus Coli Infections of the Urinary Tract especially in Relation to Hæmolytic Organisms. By Dudgeon, Wordley and Bawtree. *Journal of Hygiene.* November, 1922.—In this second investigation the authors found that two types of *B. coli* were met with in urinary infections—hæmolytic and non-hæmolytic. Hæmolytic colon bacilli were much commoner in infections of the male (seventy-two per cent) and non-hæmolytic in the female (seventy per cent). It was very uncommon to find both types associated in any particular case, or for a hæmolytic infection to be followed by a non-hæmolytic.

The fæces of eleven of the urinary cases were examined on more than one occasion for hæmolytic colon bacilli, but a positive result was obtained in only one instance and one hæmolytic colony appeared. The hæmolytic organism from the fæces was found to be different culturally and serologically from the urinary coli. The authors had expected that hæmolytic colon bacilli would be found commonly in the fæces of hæmolytic urinary cases and would correspond culturally and serologically with the urinary bacilli. At the present time deductions cannot be made as to the possibility of fæcal strains exciting urinary infections.

The hæmolytic urinary colon bacilli could be readily grouped by appropriate colon anti-sera, but this result was much less common with non-hæmolytic strains. Culture media were found to be of little value in grouping colon bacilli.

The authors have not been able to cultivate a hæmolytic colon bacillus from the blood-stream in the human subject.

Two cases of auto-infection from the urinary tract were met with. Both cases were acutely ill with high temperature and one died. The organisms isolated from these two cases were found to be similar; they fermented lactose very slowly and in litmus lactose and neutral red lactose agar plates the colonies resembled the typhoid and para-typhoid group; both strains were strongly hæmolytic and serologically similar.

The blood sera from "normal" cases and from coli infections were tested for the presence of coli agglutinins. Several coli antigens were employed. The authors note the difficulty of determining "normal" people, but found a higher percentage of positive findings with coli cases. A negative result did not prove that a case was free from infection.

Experiments with rabbits showed that these animals have a marked tolerance for large doses of living coli bacilli. Agglutinins and precipitins were readily formed by the injection of hæmolytic strains. Renal infections, however, were not produced by the injection of either urinary or fæcal colon bacilli.

The action of No. 220 soluble mercurochrome was investigated in the treatment of coli infections of the urinary tract. Young, White and Swartz used this preparation in cases of cystitis and pyelitis due to the colon bacillus and found seventy-five per cent of the former and fifty-eight per cent of the latter were cured. For the cystitis cases the bladder was washed out and then about one ounce of a one per cent solution was injected and left for one hour; this treatment was carried out twice or thrice daily for five or six days. In the pyelitis cases the ureters were catheterized and the pelvis emptied; a one per cent solution of mercurochrome was then gently instilled and allowed to remain for five minutes. The authors tried numerous experiments with mercurochrome which is readily soluble in water, forming a highly fluorescent red non-toxic solution. In some cases a cure appeared to have been effected, but on the whole the results did not reach expectation. The best results were obtained by mercurochrome lavage and vaccine therapy.

Reviews.

SYNOPSIS OF MEDICINE. By H. Letheby Tidy. Third Edition. Bristol: John Wright and Sons, Ltd, 1923. Pp. xv + 985. Price 21s. net.

The third edition of this book has been brought up to date and follows the same scheme as the earlier editions. This series appeals especially to Service medical officers because it contains a complete synopsis of medicine in a conveniently sized volume, weight being a serious consideration in selecting a medical library for service abroad. The book is greatly in advance of any other Synopsis of Medicine we have seen, and is a really useful and complete volume of reference. J. H. S.

PRACTICAL ORGANOTHERAPY. By H. R. Harrower. London: Endocrines, Ltd., 1922. Pp. 415.

Dr. Harrower has made endocrine functions and other disorders his special study for a number of years, and his books contain the results of his own and of other workers' experiences. Such a volume is welcome at the present time, even if we do not accept in full all of Dr. Harrower's conceptions. This book will serve a most useful purpose by placing before the busy practitioner certain simple tests of function which he can readily employ, and by indicating clear-cut lines of treatment and the reasons for them. J. H. S.

THE DIAGNOSIS AND TREATMENT OF HEART DISEASE. By E. M. Brockbank. Fifth Edition. London: H. K. Lewis, Ltd. Pp. xii + 232. 25 Illustrations, 3 Plates. Cr. 8vo. Price 6s. 6d. net.

This is a well known little book which explains with admirable clearness the significance and nature of the cardiac sounds both normal and abnormal. It seems a pity that the author has not included a chapter on the interpretation of polygraph tracings, because this instrument (in the absence of access to the electrocardiograph) is essential for the correct diagnosis of the arhythmias of the heart. J. H. S.

MODERN METHODS IN THE TREATMENT OF GLYCOSURIA AND DIABETES. By Professor Hugh McLean, M.D., D.Sc. London: Constable & Co. 1922. Pp. xi and 159. Price 12s.

This work which is the result of the author's original researches in the subject of Blood Sugar Content in Normal and Pathological states, forms a companion volume to the work recently published on "Renal Diseases." Pioneer work does not lend itself to criticism and in this case none is needed for the whole exposition of this difficult and complicated subject is so admirable, that we have no hesitation in saying that no one who wishes to become proficient in the diagnosis and treatment of glycosuria conditions can afford to neglect a thorough study of this book.

The interpretation of blood sugar curves and their influence on diagnosis and prognosis is especially helpful. J. H. S.

ELEMENTS OF PHARMACY, MATERIA MEDICA AND THERAPEUTICS. By Sir William Whitla. London: Baillière, Tindall and Cox. 1923. Pp. x and 678. Price 10s. 6d.

The eleventh edition of this standard work on Pharmacy and Materia Medica follows the lines of previous ones and has been bought thoroughly up to date. Especially useful will be found the section on unofficial remedies which contains a list of the new preparations which have been thoroughly tested by clinical experience and found to be of undoubted value. For the dispenser no more complete work could be desired.

Electric Ionization. Second Edition. By A. R. Friel, M.A., M.D. (Dub.)., F.R.C.S.I. Bristol: John Wright and Sons, Ltd.; London: Simpkin, Marshall, Hamilton, Kent and Co., Ltd. 1922. Pp. 132. Price 8s. net.

This small book now in its second edition, is an admirable treatise on Electric Ionization. It is of special interest in that it describes the newer electrical theories especially in relation to the nature of electricity and the direction of the flow of electrons. Some of the results claimed are a little optimistic; but in the hands of the experienced author the methods no doubt give brilliant results.

The new chapters on pyorrhœa and endometritis by the distinguished colleagues of the author are a very great help in the treatment of these diseases.

The author's references to Professor Leduc of Nantes are a pleasant and grateful tribute to the pioneer of ionic therapy.

Direct Paths to Health. By Major R. E. E. Austen. London: C. W. Daniel, Ltd. 1922. Pp. xxxv and 95. Price 5s.

On account of the essentially personal views expressed on almost every page, this book will not appeal to the large majority of medical officers. It appears, however, to have been written exclusively for the layman, who is presumably less exacting as to the validity of the arguments brought forward.

Our choice of diet, frequency of meals, and methods of breathing are called severely to account; but we are assured that, inasmuch as there is no cause of disease other than acidosis, if we mend our ways we cannot get ill. These are amongst the least startling of the facts placed before an unsuspecting public. The evidence adduced to prove the contentions is even more remarkable than the contentions themselves, and might well turn the most trusting to unbelief, unless he should be in the fortunate position of having escaped all study of medicine or logic.

The author's unique personality and his singlemindedness of purpose have undoubtedly enabled him to effect wonderful cures in certain cases chiefly, we think, by helping his clients to a measure of self control. His voice, we fear, must continue to cry in the wilderness.

Correspondence.

THE FILTER-PASSER OF INFLUENZA.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

Sir,—As Dr. Woodcock's letter in your last issue is little more than a repetition of views to which I have already replied, I do not see that much is to be gained by continuing the present discussion. Dr. Woodcock seems to have made up his mind that the filter-passing organism which I described

in the paper that appeared in the Journal of July, 1922, has not been differentiated from granules of inert protein, while I maintain that though very minute, it is a definite micro-organism distinct from granules of protein deposit, and similar in its morphology and staining properties to filter-passers previously described. I maintain further that the organism in question can be demonstrated in situ in films of the clear nasal secretion taken at the onset of influenza, and occasionally in the bronchial secretion obtained post-mortem. It grows in Noguchi medium in the manner described first by Graeme Gibson, and afterwards with more precision by Olitsky and Gates, and can be grown in subculture in the same medium.

The ætiology of influenza is far too large and important a subject to be settled by disputes on mere matters of microscopical technique: it will be solved by further evidence only to be obtained by study of the actual and experimental disease with all the methods of modern pathology. It may be of interest to mention that since the outbreak of influenza in the early months of 1922, in which I was able to confirm the work of Gibson and of Olitsky and Gates, I have examined several sporadic cases of acute catarrh that came my way within two days of the onset during the autumn and winter months of the same year, but failed in all to obtain the filter-passer either in films or in culture. These persons were suffering from ordinary mild catarrh; and their malady did not have the incapacitating effect of true influenza.

Now it is one of the blessings of bacteriology to-day, that a new point, if true, however much it may be minimized, attacked, or contradicted, is pretty sure of confirmation before very long; and such has already proved to be the case in the present instance. Thus, while influenza *vera* seems to have been in abeyance in this country recently, the disease has made its appearance in South Africa and afforded to Sir Spencer Lister (whose earlier observations Dr. Woodcock cites with approval) the opportunity of studying its bacteriology anew. As a result, Lister has succeded in confirming the work of previous observers both with regard to the presence of this minnte filter-passing organism in the acute stage of influenza, and also with regard to its morphological and cultural characters. Lister, moreover, has taken the matter a stage further by spraying seven volunteers with a culture of the organism in question, with a result that two of them developed slight pyrexia without other symptoms, while a third, nineteen hours after receiving the culture, developed an attack of typical influenza. In this last case the filter-passer was demonstrated in situ in films of the nasal secretion, and was also recovered in culture.

Nor have matters been at a standstill with regard to Pfeiffer's bacillus. Since this discussion started a monograph has been published that describes the most complete study yet made of this bacillus and of other organisms of the same hæmoglobinophilic group. The author is Dr. Martin Kristensen of Professor Madsen's Institute at Copenhagen: the work is written in English, and it should be in the hands of all who are interested

in the bacteriology of influenza. Kristensen investigated no less than 800 strains of Pfeiffer's bacillus and after examining its cultural, biochemical, and serological characters compared the organism thus defined with others of the same group. It may interest Dr. Woodcock to hear that the true Pfeiffer bacillus is not hæmolytic at all; the organism which he cites as a conceivable cause of influenza is one of the "pseudos" met with in the normal throat and dealt with by Kristensen. Having differentiated Pfeiffer's bacillus in a very thorough manner, Kristensen proceeded to determine its incidence not only in cases of influenza, but also in the general population before, during, and after influenza outbreaks. He was materially helped in this by the medical authorities of the Danish Army and Navy who allowed him to examine their recruits from all parts of the country. Nor did he confine his observations to influenza; Kristensen also examined cases of measles, whooping-cough, and tuberculosis in order to obtain a wide perspective of its incidence. He found that Pfeiffer's bacillus is as common in cases of measles as in influenza, and that in whooping cough it is even more common than in influenza. On the other hand, the Pfeiffer carrier rate in the general population rises during an outbreak of influenza, and falls when the epidemic subsides. In the light of the whole of the available evidence Kristensen does not regard Pfeiffer's bacillus as the primary infective agent in influenza. His view of its activities is briefly as follows:—

"Pfeiffer's bacillus is mainly adapted to living in the mucous membranes of the respiratory tract in man. In healthy mucous membranes it is able to exist for a time, but only with difficulty. Its ideal conditions are in mucous membranes already in a catarrhal state . . . the infection producing the catarrh is a matter of indifference—it may be influenza taken in its widest sense, whooping-cough, measles, or tuberculosis, or even catarrh due to non-bacterial cause . . . The reason why Pfeiffer's bacillus occurs most constantly in measles and whooping cough is on account of the wide distribution and endemic character of these diseases preparing the soil. The conditions are otherwise in such a pronounced epidemic disease as influenza. Consider such an epidemic in its very early stages. It spreads over a population the large majority of whom do not harbour Pfeiffer's bacillus. In the early cases Pfeiffer's bacillus is only exceptionally met with, but in those patients in whom it is present it will multiply rapidly and spread to others and also to healthy persons who may become infected with influenza as well as with Pfeiffer's bacillus, or with Pfeiffer's bacillus without developing influenza; for under the new conditions the bacillus is likely to acquire an improved capacity of developing in normal persons. . . . in this manner the bacillus spreads until it is present in almost every influenza patient and in a number of healthy individuals as well . . . When the influenza dies out Pfeiffer's bacillus may exist in normal persons for some months, but as it is incapable of living for prolonged periods as a true saprophyte it will, as before the influenza appeared, gradually be limited to occurring in endemic diseases."

This conception has the great merit that it accounts for the whole of the observed facts, and for that reason it will receive wide acceptance. While the prophylactic use of a vaccine containing Pfeiffer's bacillus finds substantial support from these comprehensive observations of Kristensen, they also offer an explanation of the uncertainty of the present procedure as a preventive of influenza. When it becomes possible to include the filter passer in the vaccine it is possible and even probable that the results may be far more satisfactory.

I am, etc.

M. H. GORDON.

REFERENCES.

LISTER: *South African Medical Record*, November, 1922.

KRISTENSEN: "Hæmoglobinophilic Bacteria." Copenhagen, 1922.

Notices.

EDITORIAL NOTICES.

The Editor will be glad to receive original communications upon professional subjects, travel, and personal experiences, etc. He will also be glad to receive items of news and information regarding matters of interest to the Corps from the various garrisons, districts, and commands at home and abroad.

All such Communications or Articles accepted and published in the "Journal of the Royal Army Medical Corps" will (unless the Author notified at the time of submission that he reserves the copyright of the Article to himself) become the property of the Library and Journal Committee, who will exercise full copyright powers concerning such Articles.

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Original Communications.

MODERN HYGROMETRY.

BY COLONEL R. J. S. SIMPSON, C.B., C.M.G.

THE title of this article should be taken as a short expression for "Hygrometry in the Twentieth Century" as there is no such alteration, either in theory or practice, as would justify a division into ancient and modern. The characteristics of present day hygrometry are: a greater knowledge of instruments and a larger field of application, especially in the related subjects of industry and hygiene, where the development of the technical applications by scientific methods is important. There is little that is essentially new, but points that in the older days had been but lightly considered are now emphasized.

(2) The object of hygrometric observations is of course the determination of the quantity of water vapour in the air, which is expressed "most correctly as a mass per unit volume, but, in practice, is more conveniently expressed by the pressure it exerts. The important meteorological matter however is the relative humidity which may be defined as the ratio of the actual amount of water vapour present in the air to the amount which the same volume of air would hold if it were saturated. It is usually expressed as a percentage" [3].

The physiologist nowadays prefers to use as an index, some quantity more directly connected with temperature, which index for the present is the wet bulb temperature. But as will be referred to later, even in hygiene when the air temperatures are fairly steady, the relative humidity which may be obtained from a direct reading instrument is a useful indication of existing conditions.

(3) The classification of instruments by their mode of operation is convenient :—

(i) Hygrometers of condensation. Dewpoint instruments.
(ii) Hygrometers of absorption. Short title: hygrometers.
(iii) Hygrometers of evaporation. Short title: psychrometers.
(iv) Chemical methods.

Of these: (ii) and (iii) are the methods in practical use because as Sir Napier Shaw says "the practical determination of humidity must be of such a nature that for regular observations it can be entrusted to an attendant who has no experience of work in a laboratory, who can follow a prescribed routine" [1]. Many instruments, both of absorption and evaporation, were made and used in the past [4]: almost every conceivable hygroscopic substance has been tried in some type of absorption instrument; some of these are still in use in the same, some in a different fashion. For instance, in 1670 Cardinal Cusano observed the absorption by dried cotton or silk, and measured the resulting increase in weight [4], while to-day the change of temperature in cotton taken from a desiccator and exposed to damp air, has been found in the laboratory to be closely related to humidity, but the method does not appear so far to be entirely trustworthy [1], and is not one for practical use. Other laboratory methods, which may become industrial, are: electrical resistance and hot wire types. A new absorption type, depending on the intake of moisture by phosphorus pentoxide, has been devised for use in cold stores. An interesting laboratory method depends on changes in the refractive index of glycerine owing to the absorption of water [1].

(4) Psychrometer, i.e., wet and dry bulb thermometer. Except when the air is saturated, the wet bulb temperature is lower than that of the dry bulb, and this difference is related to the amount of water vapour in the air at the time of observation. There is no complete theory of the instrument. Clerk Maxwell's formula for still air is rather formidable; it contains thirteen symbols. But in its simplest state, the formula is easily comprehensible, and is, $f = f' - A\,B\,(t - t')$, where

f = pressure of aqueous vapour in the air
f' = vapour pressure, saturated, at temperature t'.
t = air temperature.
t' = temperature of the wet bulb thermometer.
B = barometric pressure.
A = "a quantity which for the same instrument and for certain conditions is constant, or a function depending in a small measure on t." [5]

The point in present day hygrometry with this apparatus on which stress is laid, is the factor A which varies from one instrument to another, with their conformation, and with the velocity of ventilation of the wet bulb thermometer. But if a certain minimum velocity be ensured, these differences vanish, and all forms of psychrometer give comparable results. The velocity of ventilation for which the Smithsonian tables are computed

is a minimum of three metres per second (6·7 miles per hour). Principal S. Skinner [1] gives a diagram showing the value of this constant at various velocities, where there is a further slight fall to a velocity of four metres per second (8·9 miles per hour).

There is no doubt that the unventilated wet bulb thermometer is a "notoriously unreliable instrument" [1]. If the air is quite still and a reading taken (using the ordinary domestic pattern of wet and dry bulb apparatus with the bulbs at most only a few inches apart), and the instrument is then swung so as to ventilate it properly, it will often be found that the temperature of the dry bulb rises a fraction of a degree, while the wet bulb always falls still further.

The methods by which the necessary ventilation may be obtained range from the very simple to the rather complicated. The thermometer may be attached to a string one metre long and swung round about once per second; more conveniently in the house, the two may be attached to a sort of rattle and rapidly revolved. For constant observation in observatories, meteorological stations and industrial concerns, mechanically ventilated psychrometers, such as Assmann's, are satisfactory if the velocity of ventilation is tested from time to time.

Certain sets of tables are calculated for definite rates of ventilation (the Smithsonian for 3 metres per second), and in some, factors are given by which the results as found in the tables may be converted to those corresponding to other conditions. Jelinek's psychrometer tables are computed for a wind velocity of 1 to 1·5 metres per second, the so-called "light winds," but factors are given for "calm," velocity 0 — 0·5 metre per second, and for "strong winds," above 2·5 metres per second. "Calm air" includes exposure in the open or in rooms; "strong winds" includes observation with sling instruments or with Assmann's psychrometer.

It is interesting to note that the necessity for ventilation was recognized as long ago as 1830, and was put in practice by Espy [5]. The change from unventilated to ventilated instruments at all meteorological stations is a serious matter, but there is no reason why individual observers, especially those taking up any special investigation, should not use the better method, which has been adopted by some physiologists. Whipple [1] thinks that the "light wind" formula as used in Jelinek's tables should be used for the unventilated type of instrument, and that Glaisher's method should be given up.

The psychrometer has a small relative error: compared with a dew-point apparatus it showed differences of one to two per cent in the relative humidity [1].

(5) Hygrometer: Of all the absorption hygrometers made of organic materials, hair, catgut, wood, whalebone, etc., the hair hygrometer is the only one now in use. It has two advantages as compared with the psychrometer: it is as accurate about the freezing point of water as at any other temperature; under those conditions the psychrometer is useless owing to

the "uncertain physical condition of water just below the normal freezing point," Shaw [1]. Further, it is direct reading and gives the relative humidity by inspection.

De Saussure first used in 1780 a single human hair; his instrument is illustrated in the catalogue of the collections in the Science Museum, Meteorology, 1922 [4], and the type is excellent. Later types contained as many as eight hairs, and some modern forms what might be called a bundle. In these forms, the change in length measured must be the general average of the group, while if the single hair is properly weighted, it is more delicate in its indications, more satisfactory, though more liable to injury.

Griffiths [1] made a careful comparative study of three hygrometers, using a dewpoint apparatus and an Assmann's psychrometer as controls, the two control instruments agreed within two per cent practically for all observations at room temperature. The hair hygrometers showed divergencies of the order of four per cent with an occasional departure up to ten per cent, possibly due to lag in settling to the equilibrium value after a rapid change of humidity; the divergencies occurred on all three hygrometers simultaneously. Their absorption varies but little with the temperature; the readings tend to be too high as time goes on. At very low humidities, of the order of five per cent, a permanent change occurred indicating a permanent stretching of the hairs, and a similar result occurred after exposure to low temperatures. The most serious defect of this type of hygrometer is the permanent change on exposure for the first time to big fluctuations of temperature or humidity which resulted in an elongation of the hair; this "may possibly be due to the fact that the hairs are always under tension by the control springs." Later [5] relief of tension between observations is suggested, a matter which should be easily arranged. Scales of commercial instruments are incorrect, because calibration was made with an unventilated psychrometer. In one hygrometer with gravity instead of spring control, the maximum error at room temperature, between 50 and 100 per cent humidity, was only 3 per cent. Generally the best results are obtained from instruments where the extending force is small and the mechanical parts well made.

Whipple [1] deals with the theory of the hygrometer in an interesting paper; he points out that theory and experience agree that it should not be used in a very dry atmosphere; the range from 100 to 20 per cent should suffice for all purposes, further that it would be better to return to de Saussure's original method of graduation, where equal intervals correspond to equal extensions of the hair, when the humidity value can be read off a table or graph.

Calibration in vessels containing various strengths of sulphuric acid and so giving definite vapour pressures, was originated by Gay Lussac before 1783, continued by Regnault (1845) and has now been further developed, so that a range of humidities from ten to ninety per cent at

temperatures from 0°—90° C. are now available. An Abbe refractometer with glycerine may also be used. Slower methods, by comparison with a dewpoint apparatus, or a psychrometer, may also be employed.

At low humidities, observation or calibration is affected by the presence of the observer in the room.

According to theory, the contraction of the hair from its length when saturated (at ninety-five per cent relative humidity) is proportional to the logarithm of the relative humidity, so that assuming this to be correct, two observations will suffice to calibrate the hygrometer Whipple [1].

Control over temperature and humidity is reçognized as essential in such industrial processes as the seasoning of timber, the drying of raw rubber, and the preservation of foodstuffs by cold storage; it has long been practised in the manufacture of textiles. In relation to cold storage, both for convenience and on account of the influence of the body of the observer on the result, either with psychrometer or hygrometer, distant reading and continuous record types of instruments have been developed; these are not without ancestors.

(6) Absolute and relative humidity are related in that absolute humidity represents the volume of water or the vapour pressure actually present in the air, while relative humidity represents the relation of the quantity actually present (the absolute humidity) to the maximum volume or pressure which is possible at the air temperature. The method of obtaining these quantities may be briefly referred to with subsequent advantage.

The tables used give the vapour pressure required to saturate the air at the temperatures shown against each pressure. The symbols used to represent the various temperatures and pressures are as follows: the dewpoint symbol is not always the samé.

Temperature of air t corresponding vapour pressure f.

Temperature of wet bulb t′ corresponding vapour pressure f′.

Temperature of dewpoint T corresponding vapour pressure F.

Quantity corresponding to (t—t′) is q.

The two thermometers having been read:—

(A) Opposite the wet bulb temperature (t′) in the tables is found the vapour pressure at that temperature (f′).

(B) From another table, opposite the difference between the dry and wet bulb temperatures is found a quantity (q) expressed in the same units as the vapour pressures, which has to be subtracted from the vapour pressure as found in A.

(C) The quantity left after this subtraction (t′−q) is the vapour pressure in the air at the time of observation (F), i.e., it gives the absolute humidity. It is also the saturation pressure at a certain temperature lower than that of the wet bulb, such that any further depression will result in condensation of a part of the vapour. That temperature is the dewpoint (T).

(D) Dividing this quantity (F) by the pressure found in the tables opposite the *air* temperature (t) and multiplying by 100, we have the relative humidity per cent. Further findings are :—

(E) The quantity found under (C), that is F, is less than the saturation pressure (f′) at the wet bulb temperature by the quantity (q) subtracted under (B). This is the "saturation deficit," preferred to the relative humidity by some meteorologists: it may be left as an absolute number, or expressed as a percentage of the pressure at the wet bulb temperature.

(7) If vapour pressures as given in the tables are plotted as verticals against the corresponding temperatures on a horizontal axis, say from left to right, the resulting graph is concave upwards, rising steadily to the right, i.e., the vapour pressures increase more rapidly than the corresponding temperatures. On the other hand the quantity subtracted (q) which is determined by the difference in temperatures, grows steadily with the increase in this difference irrespective of temperature: whether the dry and wet bulb temperatures are 90° and 85° F. or 50° and 45° F., the quantity to be subtracted remains the same (0·054 inch). But as the vapour pressure has been increasing more rapidly, between 85°—90° F. (0·607 inch) than between 45°—50° F. (0·062 inch) the relative reduction of the wet bulb pressure is obviously less in the first case than in the second, and the relative humidities are respectively 81·5 per cent and 67·5 per cent. With an air temperature of 50° F. to obtain a relative humidity of 81·5 per cent, the wet bulb temperature would be 47·5° F., a difference of 2·5° F., instead of 5° F.

The most suitable form in which to express the results of the observations depends largely on the purpose for which they are made. The relative humidity is a most valuable quantity for the meteorologist, though unsatisfactory for the physiologist. Hann and some others prefer the saturation deficit: this appears to be more suitable for use in hygiene than the relative humidity. Either of these forms gives very definite information, especially if considered with the collateral evidence from the actual temperature readings. Further, where the air temperatures vary only slightly, as in some industrial processes, the observation of the relative humidity, as given by a direct reading instrument, gives all the necessary information very easily. Where, as in matters of hygiene, this is insufficient, we can obtain what we require from the relative humidity plus one temperature, either air or wet bulb, and then, except with very low humidities, we can, by simple inspection of suitable tables without calculation, get whatever we want. A combination of a hygrometer with a dry bulb thermometer, which can be obtained commercially, is very convenient: it should be suitable for several types of investigation.

Physiologists, following Haldane's authoritative statement that it is the wet bulb temperature that matters have adopted this temperature as the standard of reference. It has the great advantage that

the danger points, the bearable limits of temperature in relation to humidity and rate of ventilation, have already been established by Haldane and others, and are well known. It is easy to observe, and observation of the air temperature at the same time gives very little additional trouble; whether or not the double results are worked out by the observer, they should certainly be recorded for the use of others who may prefer different methods of presentation.

The Indian Meteorological Department now publish the wet bulb readings[1] in addition to the information usually given.

Dr. G. C. Simpson, F.R.S., Director of the Meteorological Office, says [8]: "There can be no doubt that in hot countries at least, and in the absence of katathermometric readings, the wet bulb temperatures afford the best means we have of determining the degree of comfort of any particular locality." This memoir [8] should be read by every one interested in this subject.

The wet bulb thermometer suggests an obvious analogy with the body, at least the perspiring body, in that both are cooled by evaporation. That is, the thermometer bulb attains a steady temperature when the heat received by it from its surroundings is equal to that given out by it in evaporating the water in its jacket. Its "temperature is a definite characteristic of any mass of air and has a definite meaning as a heat function" [8]. It indicates the vapour pressure which cannot be exceeded under the existing conditions. Other bodies in a similar physical condition attain the same temperature. Obviously the body, as a source of heat, does not fulfil the limiting condition, but the wet bulb temperature is a sufficient indication of the vapour pressure which may be reached by evaporation, and hence of the degree of cooling of the body which is possible.

It should, however, be remembered that the thermometric scale is an evenly divided scale: one degree has the same value at any part of the scale. On the other hand, as has been pointed out, the vapour pressures, which is what we are really desirous of dealing with, increase more rapidly than the temperature, so that if we have two sets of wet bulb readings where the difference in degrees is the same in each, but where the readings are situated on different parts of the thermometric scale, we have no equality in the difference of pressures, in spite of the equality in the temperature differences. Taking a constant difference of 2° F., the difference in pressure to 45° to 47° F. is 0·024 inch, 55° to 57° F. is 0·033 inch, 65° to 67° F. is 0·045 inch, so that in 20° F. the difference in vapour pressure has about doubled. Where results are plotted, first against degrees and secondly against vapour pressures, the flattening of the graph is very definite. So that in using degrees of the thermometric scale only, we are introducing, to a smaller extent, the same uncertainty

[1] These readings, however, are from unventilated psychrometers.

that arises in the use of relative humidity. Fortunately the remedy is easy: it consists in plotting the vapour pressures, and numbering the values by the degree on the thermometric scale which corresponds to each pressure, using of course appropriate intervals.

(8) An important question arises: are the known errors of the various instruments, psychrometer or hygrometer, greater than the least difference producing definite effects on the body? There does not appear to be any information which would enable a definite answer to be given to this question. Hann [7] quotes Pettenkofer and Voit to the effect that a difference of 1 per cent in the relative humidity has an appreciable effect on evaporation from the body: very strong evidence appears to be needed to support this view, in spite of the undoubted authority of the authors quoted.

The following diagram shows the results obtained by Rubner and by Wolpert (taken from tables given in the "Science of Ventilation," etc., Medical Research Committee, Part I, 1919 [2]). Their tables give the air temperature, the relative humidity, and the water evaporated in grammes per hour: from the air temperature and the relative humidity, the wet bulb temperature and the other consequent details have been easily obtained, mostly by inspection, and the evaporation in grammes has been plotted against each of the quantities which are obtainable from the information given. The diagram shows that result which is undoubtedly the most useful, i.e., where the temperature scale at the bottom is that of the wet bulb thermometer, plotted, as suggested above, in terms of the vapour pressure, which is also shown. The lengthening of the space needed for each degree as one passes to the right shows the more rapid increase of pressure than temperature which has been referred to already.

In each case, the subject, a man, did work at the rate of 15,000 kilogramme metres per second at the various temperatures: in each his normal output of water by evaporation was forty-two grammes per hour at 20° C.; in Rubner's case, he worked in still air in a chamber; in Wolpert's, no detail is given, but probably this condition also obtained.

Wolpert's results appear consistent, the graph is such as would be expected (expectation is not always a safe guide), but Rubner's results are certainly inconsistent among themselves.

In view of the great discrepancies between the two graphs, especially at the ends, the only part that deserves consideration now is that between about 11·5 C. and 16° C., which is practically common to both graphs. Taking the actual figures :—

Rubner. Difference of temperature 16·2° − 12·6° C. = 3·6° C.

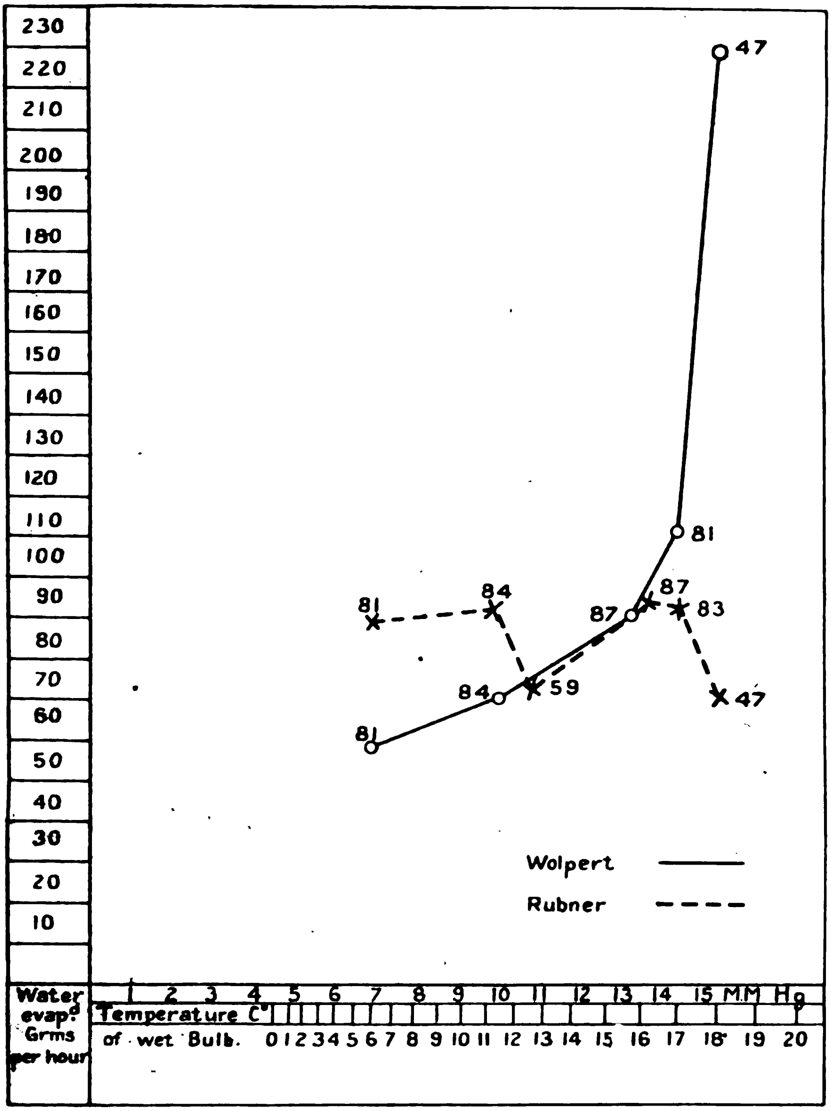
Difference of output 94·4 − 73 grammes = 21·4 grammes, or an increase in output of 5·94, say 6 grammes, per 1° C.

Wolpert. Difference of temperature 15·7° − 11·5° C. = 4·2° C.

Difference of output 90·4 − 70·8 = 19·6 grammes, or an increase of 4·67, say 5 grammes, per 1° C. In both cases the observations were con-

secutive; in Rubner's at humidities of 59 and 87; in Wolpert's of 84 and 87 per cent. This probably explains the greater output in Rubner's case.

In Rubner's case the mean output was 83·7 grammes, on which 5·94 grammes gives a percentage increase of 7·1 per cent. for 1° C. In



Wolpert's case the mean output was 80·6 grammes on which 4·67 grammes gives a percentage increase of 5·79 per cent., so that taking the mean we may conclude that about these temperatures and humidities a difference of 1° C. wet bulb temperature is equal to a difference of about 5·9 per cent. in output. At these temperatures, a difference of 1° C. in the temperature

of the wet bulb equals about $\pm$ 12 per cent. in the relative humidity, so that errors in the instruments employed in observation, amounting to 2—4 per cent. in the relative humidity, would appear to be negligible.

The corresponding relative humidities are placed against each observation in the graphs; there are curious coincidences and differences.

(9) Another method of estimating the cooling powers of the air cannot be passed over, though in strict terms it is not a hygrometric process. L. Hill [2] has developed the large bulbed alcohol thermometer into an accurate measuring instrument; it is well known as the katathermometer. It consists of two similar thermometers, one used dry, the other is provided with a silk-meshed jacket. In use, each is heated in water to above 100° F., and the time taken by each to cool from 100° to 95° F. is noted. The time taken for the cooling of the dry kata gives a measure of the total heat loss of the instrument by radiation and convection; the wet kata gives that by radiation, convection and evaporation, so that the difference between the two gives the loss due to evaporation alone. Each thermometer has a factor engraved on the stem, and by dividing the time of cooling by this factor, we get the cooling power in millicalories per second. A very complete account of its capabilities and use is given in the "Science of Ventilation," Medical Research Committee, 1919 [2]; indeed this volume contains a mass of information on all the points which may arise in the consideration of the effect of heat, or moisture, or both, on the body, so much indeed that it is difficult to bring all the various details to a common focus.

The katathermometer is young as compared with other methods; the information it gives, however important, is expressed in other units, and it is not clear how far it may be possible to fit the results of observations made with the kata into the mass of existing records. It would appear to be invaluable to the individual observer, but unsuitable for routine use.

(10) Various attempts have been made to correlate climatic elements with individual feelings of comfort or discomfort, or to obtain a general expression for all climatic elements. Hann [7] and Hill [2] give much information regarding these attempts, which have not been very successful. It is difficult to see how any correlation between personal feelings and climatic elements can have any success: we probably all agree about extreme conditions, otherwise our feelings depend as much on the mood of the moment as on any obvious physical factor.

REFERENCES.

[1] "A Discussion on Hygrometry." Physical Society of London, 1921.
[2] "The Science of Ventilation, etc," Medical Research Committee, Part I, 1919.
[3] "A short course in Elementary Meteorology." Meteorological Office, 1921.
[4] "Catalogue of the Collections in the Science Museum." Meteorology, 1922.
[5] "Smithsonian Meteorological Tables," 1907.
[6] Jelinek's "Hygrometer Tafeln," 1911.
[7] Hann's "Klimatologie," 1908.
[8] "Memoirs of the Indian Meteorological Department," vol. xxxiii, Part I, 1921.

A WORD FOR MULE LITTERS, AND HOW TO CONSTRUCT ONE OF BAMBOO.

By Major P. G. M. ELVERY, D.S.O., M.C.
Royal Army Medical Corps.

In the Journal of the Royal Army Medical Corps for November, 1922, there appears a very interesting article on the March into Kurdistan during the Rebellion of 1919, by Captain J. V. Burns, R.A.M.C.

In this article he mentions the conveyance of sick and wounded by dhooley bearers, cacolets, and spare pack mules, and last, but not least, by a mule litter made of bamboo.

My chief object in writing this article is to supplement the favourable remarks made concerning the last-mentioned conveyance, appreciating at the same time its disadvantages on very precipitous and tortuous mountain passes.

I consider, however, that the mule litter has great advantages, within a well-defined limit, over all other forms of casualty pack transport, and within this limit of its usefulness it is desirable that the advantages the litter possesses should be well known.

This form of conveyance dates back many years, and it is interesting to note that in the reign of Charles II a certain Major-General Skipton came, when wounded, in a horse litter to London. Later, in January, 1860, the United States Army Medical Board ordered horse litters to be prepared and furnished to posts where they might be required for service on ground not admitting the employment of two-wheeled carriages. The side poles of these litters were made of ash, and each pole was sixteen feet long.

Present-day general opinion on the subject has a tendency to look upon conveyance by mule litter as a very doubtful expedient, the chief cause of adverse criticism being the unprofitable expenditure of labour, i.e., the carriage of one casualty by two animals, and the necessity of having a driver for each mule with a R.A.M.C. orderly in charge of the patient.

In actual fact, one lying-down case and two sitting cases can be carried, the latter astride over blankets on the front and rear mules. This does not throw an impossible load on either mule for short distances. Experiments have proved also that, given suitable material, two lying cases can be carried on specially broad litters made to take two stretchers.

The question arises: Can the conveyance of serious casualties over ground unfit for wheel transport be more economically accomplished by other means? In all probability, in the not too far-distant future, the difficulty will be solved by aeroplane and caterpillar-wheeled motor ambulances.

In the meantime the comfort of the patient, reduction of shock to a minimum, and the prevention of damage to the wound, should be the first

consideration, and not economy in mules and personnel; and until some other means of transport is devised, mule litters, and especially those made of bamboo, in my opinion, should stand in the front rank of definite casualty equipment for all ambulances which are likely to be placed on a modified or full-pack basis.

In Macedonia during 1916, when ambulances on a modified pack scale were introduced, mule litters (ten in number) were included, and these proved invaluable over marshy and other terrain impossible for wheel transport.

At one period of 1916, during the winter months, a number of mule litters from each ambulance of the Division were brigaded and used in the evacuation of gunshot wounds of the thigh with fracture, and other serious casualties, from a casualty clearing station on the Salonica-Seres Road. This section of the road was so badly cut up with heavy traffic as to be unsafe for motor ambulances, and it was quite out of the question to subject the wounded to the unavoidable jolting entailed by wheel transport.

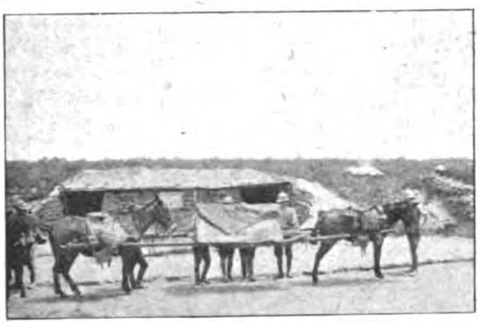


Photo. 1.—Loading an Ash Litter at an A.D.S. in the Struma Valley.

Photo. 2.—The first Travois.

These litters were made of ash. The poles and cross-pieces, with bolts and clamps, were issued by the R.A.O.D., and were put together by the personnel of field ambulances according to specifications supplied. When completed, they were a colossal weight, and were eventually cut down and otherwise lightened and altered.

At the same time travois (a conveyance similar to the litter, except that the rear shafts are not supported by a mule, but pulled along the ground and made of unequal lengths to minimize jolting) were constructed, and given a fair trial, but proved very unpopular.

Breast harness, breechings, and traces were used with these two forms of conveyance, but in the case of the litter were quite redundant—a ring attachment to the hooks of the pack-saddle being all that was necessary. (See note and drawings at end.)

Mule cacolets for two sitting cases were also used. They were, however,

more often dumped on account of their weight (fifty-six pounds). The combined weight of cacolets and patients proved so great that only strong mules could be used, and these were not always obtainable.

Ultimately a proportion of universal riding saddles (twenty) were issued in lieu of twenty pairs of cacolets, and they, with the ash litters, became the main casualty collecting conveyances during the winter months.

The training of mules for litter work needed a little patience, but, given suitable animals and intelligent men to train them, there was never any difficulty. Animals that were accustomed to draft work turned out the best litter mules.

Captain Burns mentions in his article the difficulties he experienced in negotiating sharp corners. I can well understand this, but as every form of casualty conveyance has its limitations, the litter should not be condemned on this account.

PHOTO. 3.—Mule Cacolets.

In my own experience in Macedonia and Palestine I have never known a litter to off-load on account of a bad corner—although this can be easily done provided the patient is lying on a stretcher.

It was also found that the steepest of hills could be traversed by the litter that was carefully piloted. By being carefully piloted, I mean the use of a little common sense on the part of the driver.

A mule should be led, and not dragged, and if it is possible to avoid a very steep incline by taking a zig-zag course, or a more circuitous route, this should be done.[1]

In Palestine I had my first experience with bamboo litters, and in my opinion they were a great advance on those made of ash, being very much lighter, an important consideration from the point of view of man-handling.

Mule litters were hardly thought of in the Holy Land until field ambulances found themselves confronted by the hills of Judea.

From Rafa almost to Beersheba, and from thence to Gaza and Latrone,

[1] A great deal of very useful information for field ambulance commanders *re* mules may be obtained from R.A.S.C. Training, Part III, 1922.

the desert casualty transport, which consisted of sand-carts, Ford motor ambulances, and camel cacolets, both sitting and lying, fulfilled all requirements. Although the last-mentioned forms of conveyance did yeoman service, they were suitable for unconscious patients only.

PHOTO. 4.—Camel lying Cacolets. Note stony ground.

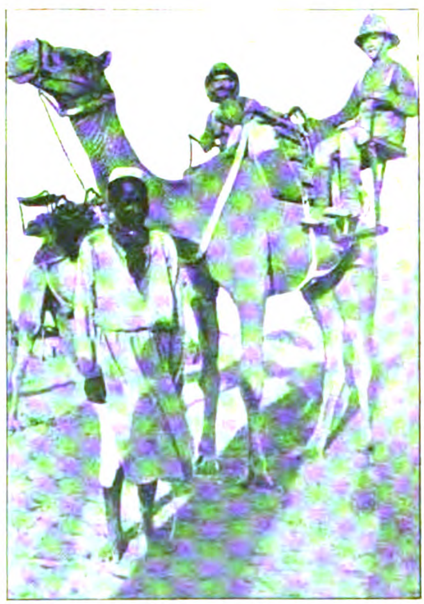


PHOTO. 5.—Camel Cacolets, sitting cases.

At Latrone it became apparent that immediate alteration in casualty transport was necessary, owing to the condition of the ground ahead, which seemed to present nearly every kind of difficulty. Roads were practically

PHOTO. 6.—Sandcart with spring mattress for two lying cases.

PHOTO. 7. Typical country before Judean Hills were reached.

PHOTO. 8.—Judean Hill country.

non-existent, and the ground was boulder-strewn, precipitous, and very slippery after rain.

The sandcarts were withdrawn, light ambulance wagons replacing them; tracks to the advanced dressing stations were improved, and every-

PHOTO. 9.—First attempts with Bamboo Litter.

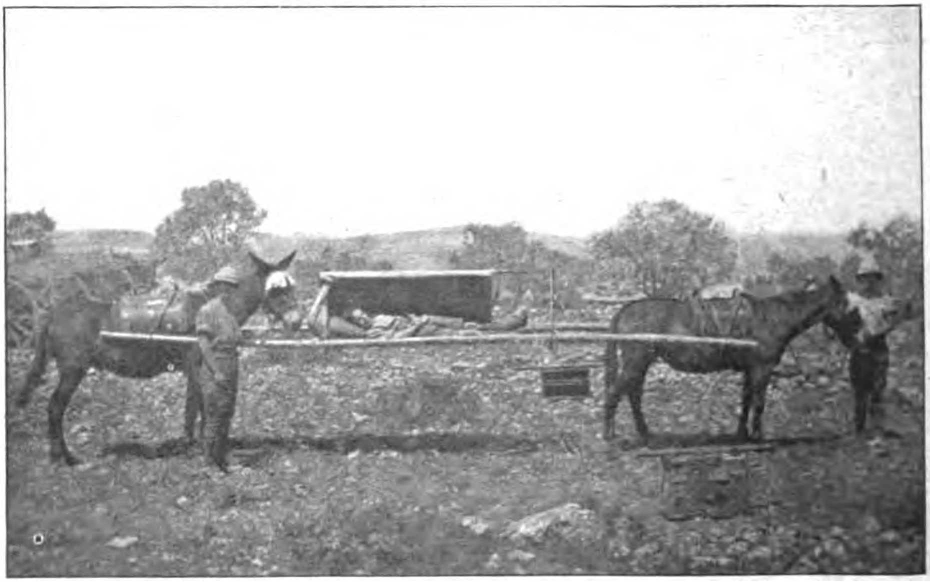


PHOTO. 10.—The Bamboo Litter as finally completed. The detachable Litter Cover open on one side.

thing was done to assist camels in their arduous duties. After a short stay at Latrone we moved forward to Um Suffa, where the winter rains set in, and to make all difficulties worse, roads and tracks that had been made in the waddis were washed away, and gradients were rendered more dangerous,

and almost impossible, for camel casualty transport. It was therefore decided to construct bamboo mule litters for immediate use. These were made according to the specifications which were drawn up by the unit, and are given at the end of this article.

Bamboo poles that were neither worm-eaten, sun-dried, nor cracked, were difficult at first to obtain, but our R.E. officer never forsook us or refused a request.

To obtain mule pack-saddles was the next difficulty, and until they arrived the litters were carried in leather loops attached by rope to the front and back arches of the universal riding saddle, stops being placed on the litter poles to prevent them slipping out. (See photo 9.)

When the ambulance moved forward, these litters were carried on the wheel transport. From January until the end of May, 1919, camel cacolets ceased to be used in the ambulance for the conveyance of the severely wounded, all such cases being transported by mule litter. In April, 1918, following a lengthy correspondence, a practical demonstration to the D.D.M.S. of the Corps (he being the patient), and finally another demonstration at General Headquarters, bamboo poles and pack saddles were authorized as an issue to Field Ambulances.

Photograph 10 shows the litter as finally completed, and all information necessary for its construction is given in the Appendix.

APPENDIX.

BAMBOO MULE LITTER.

For Conveyance of One Lying Case.

Material required for Construction of One Litter.

Poles bamboo (*new*), 20 feet	2
Crosspieces wood, 3 ft. 6 in. by 3 in. by 2½ in....	2
Wire netting, small mesh	8 feet
Tibbin binding, for tug stops	20 feet
Wire, for binding crosspieces to shafts ...	20 yards
Cordage, 1½ in.	8 yards
Leather or web, 2½ in. wide	6 feet
Rings iron	12

Notes.

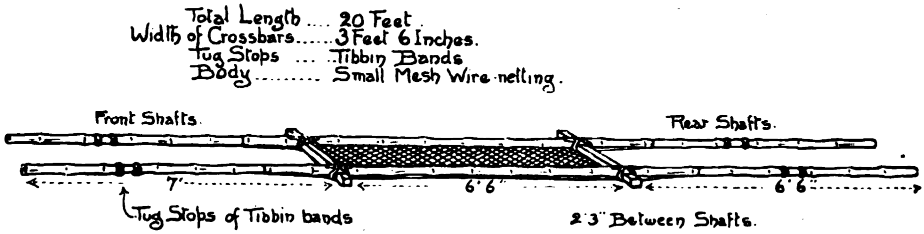
Stretchers or camel cacolets can be used as litter beds instead of wire netting, but the latter minimizes weight.

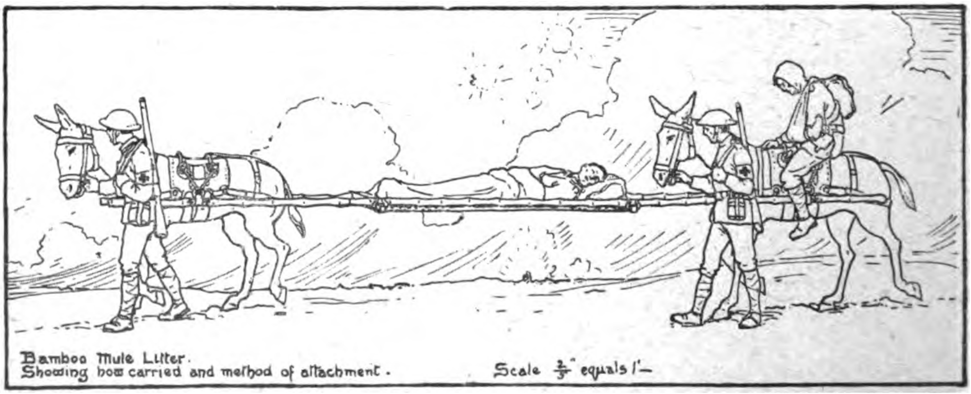
Chains, adjustable, 4 *feet with centre ring,* are just as suitable as cordage for suspending the litter from the pack-saddle.

The weight of one litter constructed to the above specification is approximately 56 lb. Loaded with an 11 st. patient and 50 lb. kit, the *total load* for two miles is 260 lb., or 130 lb. *each mule*.

The Easily Detachable Litter Cover.

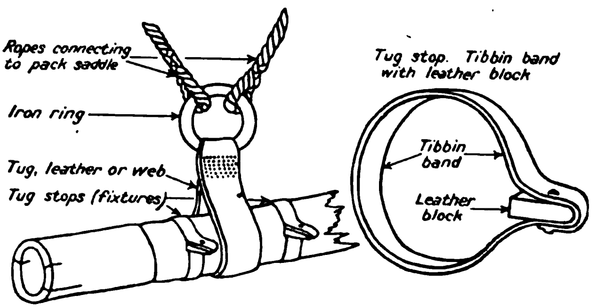
Sheets bivouac	One and a half
Wood (deal) for ridge bar 7 ft. 3 in. by $1\frac{1}{2}$ in. by $1\frac{1}{4}$ in. ...	One piece
Wood (deal) for uprights and sockets 6 ft. by 3 in. by $\frac{3}{4}$ in.	One piece



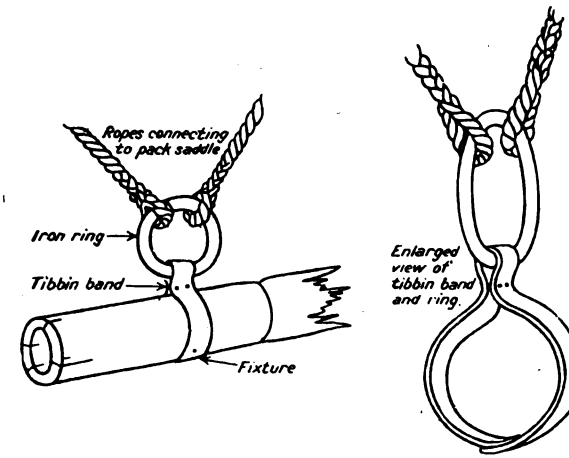

Bamboo Mule Litter.
Showing how carried and method of attachment.
Scale $\frac{3}{8}$" equals 1'—

LITTER CARRIERS—Two Forms.

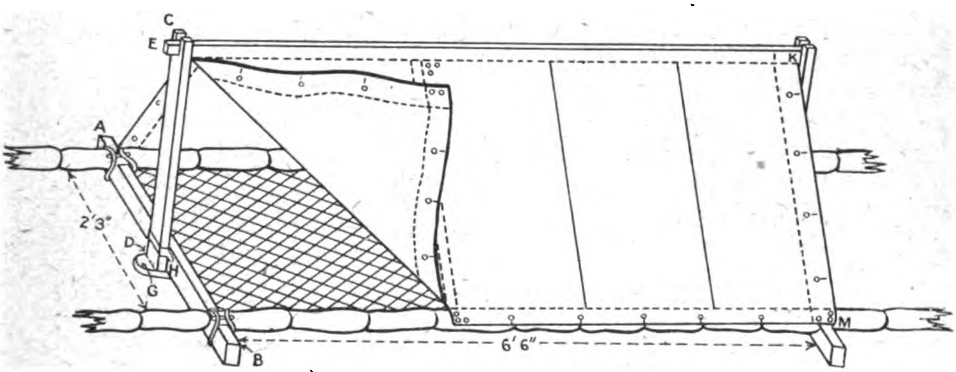
No. 1.—The "Quick Release" Litter Carrier.



No. 2.—The "Ring-Attachment" Litter Carrier.



The Easily Detachable Litter Cover for Protection against Sun and Rain.



Measurements:—A to B 3 ft. 6 in.; C to D 2 ft. 9 in.; E to F 7 ft. 8 in.; D to G 3 in.; G to H 2 in.; K to M, 2 ft. 9 in. Cover is made with one and a half bivouac sheets. Weight of litter cover complete with supports is 6½ lb.

ARMY HYGIENE ADVISORY COMMITTEE REPORT No. 2.

An Investigation on the Motion Study of Digging and the Energy Expenditure involved, with the Object of increasing Efficiency of Output and economizing Energy.

By Captain A. G. STEVENSON.
Royal Army Medical Corps.
and
Captain R. L. BROWN.
Royal Engineers.

Introductory.

The following is an abbreviated account of a Report, submitted to the Royal Engineer Board and to the Army Hygiene Advisory Committee, of an investigation on Trench Digging undertaken at the request of the Royal Engineer Board. All unnecessary details and tables of figures have been omitted, along with various graphs, but a few tables have been retained to show how the various experimental results were tabulated and graphically represented.

The work of cutting-down the Report of an original investigation, and rendering it in such a way as to present its original form with the gradual working up to a definite conclusion, and yet at the same time to omit no necessary link in the chain which was forged in arriving at that conclusion, has proved a task which has not been lightly undertaken.

Of necessity a large amount of the subject matter of a report on Picking and Shovelling will approximate to the "dry" nature of the work involved, but it is hoped that the following account of an investigation lately carried out will show how the introduction of new ways of performing a very common type of work brought about an economy of effort along with an increased production, and how adherence to true physiological and psychological principles is necessary if body and mind are to be utilized in the most economic way. From the military aspect, many of the every-day tasks of the soldier are capable of being investigated with profit to the Army as a whole, and we hope that this account of the investigation of one of his tasks will stimulate thought to the many further problems which present themselves.

In the limited time at our disposal and owing to the uncertainty of the weather conditions during the winter months these investigations were necessarily based on lines which would be likely to produce he best results in the shortest time.

No pretension has been made to make an exhaustive research into the many interesting channels which opened up as the work progressed, and as a consequence conclusions have been formulated on certain lines of this

work which need corroboration and would require further research to justify their definite adoption. This applies particularly to the Rest-Pause Section and to the experimental work done on different sized shovels.

During the last few weeks of the Investigation most of the experimental work on Energy Cost had perforce to be carried out on the authors of this report, thus limiting the benefit which would be derived from taking an average of the results of many subjects.

In our opinion the undertaking of a further and more detailed research on the whole subject, would be amply justified.

(1) Previous Work.

From both the military and civil points of view "digging" is of paramount importance. Although various forms of machinery have been devised and are at present in use, digging is a form of work which has neither been diminished in its practical form nor supplanted in many of its uses by man's inventive genius.

No doubt machinery has replaced the individual toiler in many tasks, but certain forms and types of digging and shovelling work can never be so done away with.

This is certainly an established fact from the military point of view. An army on the offensive or defensive has to rely on the individual soldier to obtain its cover from fire and to establish defensive points. Types of machinery have been devised for trench digging purposes, but no matter how efficient or reliable they may be there are certain definite conditions which oppose their general use: the difficulty of transport, the all important factor of speed and being on the right spot when wanted, the changing character of the terrain, and the uncertain nature of machinery as opposed to the human machine—all these tend to show that for defence purposes man cannot be supplanted on the field of battle by mechanical means but must dig out his own salvation.

The importance of digging was made very apparent during the late war (1914—1918), and one cannot see any likelihood of this importance tending to diminish in any war in the near future. No matter how quickly aircraft and tanks are developed as prime factors in the wars of the future and how open the warfare may become, the deciding factor eventually is the individual soldier, and on occasions he must "get down" and get down at once and in the quickest time possible if casualties are to be prevented.

He must also carry his own means of defence or have it brought up behind him. Certainly, when the necessity arises his pick and shovel must be at hand, and the greater his efficiency in these tools, the greater his chance of safety and as a result the ultimate success of his country.

It is a remarkable fact that, with the exception of one or two researches

on shovelling by American workers, no serious investigation of digging has been undertaken.

Despite the absence of scientific data, definite standards for the weight, length, and type of pick and shovel have been laid down and adhered to, definite methods for their use have been drawn up, and all on purely empirical grounds.

It is part of the work of the present investigation to find out how near these standards approach the optimum from the military point of view.

The previous work done by American investigators goes to prove that shape and size of tool, and style and methods employed in their use have a most important bearing (as one would expect) on efficiency of output.

Thus Hauer [1], experimenting with different types of shovels in different types of soil, found that alteration in the shape and size of the shovel increased efficiency and also that the rate of throw and the method employed altered with these different factors, and optimum rates and methods could be determined in the varying conditions. He laid down standards for the varying types of soil and of throw, horizontal and vertical. His figures are of extreme interest, and for the various occupations where they can be used with advantage, e.g., civil occupations where conditions are not likely to change to any extent, must undoubtedly produce an economy of time and labour.

From the military point of view the practical difficulties in the way of establishing various sizes and standards of tools on account of the uncertainty of local conditions, the ever-present question of transport, and the question of economy bring us down to the real issue—to supply a pick and shovel which will be the most efficient on the average on all types of soil; which, in relation to the average physique of the individual will be the best; and which, as regards weight and size, will be easily carried.

Again as regards methods of use and rate of work, the aim is to evolve a method which will be the optimum under all conditions, will suit the average soldier best, and will reduce fatigue and increase speed of output.

Hauer's work, therefore, though valuable in its findings, cannot assist us to any extent.

Gilbreth [2] who has done much valuable work on motion study, and who, more than any other worker, raised this science to its present state of importance, has no published results of any work on digging, but his theories and practical methods have given us great assistance in investigating the problem.

Taylor [3], investigating the shovelling of coke, laid down the optimum type of shovel, the economic load, and the optimum rate, and claimed that as a result of the methods in use being changed in accordance with his ideas, efficiency of output was increased by thirty per cent, resulting in a saving of time and an economy of labour. To what extent fatigue resulted from these new methods was not investigated, and one cannot be certain that his standards could be continued indefinitely without decreasing efficiency owing to gradual prolonged overstrain.

(2) Reasons for the Present Investigation.

Briefly the reasons and objects of the present investigation as described by Major-General Thuillier, C.B., who initiated the investigation, were as follows:—

(1) During the late war the skill in digging displayed by the average British soldier was of a poor type and was lacking in uniformity.

(2) The Germans, on the other hand, having a training in this work, were infinitely more skilled.

(3) No recognized digging drill existed in our Army.

(4) Our Expeditionary Force which crossed to France in 1914 has been recognized by all as having been the most efficient force with regard to rifle firing which has ever existed. This efficiency was produced by training and was not naturally acquired. Trench digging, on the same lines, could be taught and brought to the same pitch of efficiency by instituting a training on proper lines.

(5) Accordingly a digging drill based on common-sense principles with due regards to the methods of skilled navvies was introduced as part of the peace training of recruits in the Royal Engineers.

The results of this drill were found to be of so much value that it was considered that a complete investigation of the whole problem would be likely still further to increase efficiency in digging.

(6) To test the value of this drill on scientific lines, to test the efficiency of the present Army tools in relation to the average individual stature of the soldier, and to evolve, if necessary, methods and tools for digging purposes which would produce the maximum output with the minimum of fatigue—were the terms of reference of the present investigation.

(3) Nature and Methods of Investigation.

At the outset, after a preliminary study of local conditions, methods, material, and personnel had been made, a plan of investigation was drawn up and submitted for approval. This approval having been received, the work, of which the present report is the result, was proceeded with.

Plan of Research.

This as originally drawn up was adhered to in the main principles, though as the work proceeded various side-lines presented themselves and had to be tackled in their turn.

Briefly, the investigation was conducted on the following different lines:—

(1) *By Empirical Methods.*—By observing the methods of both skilled and unskilled workers to eliminate obvious faults in style and the use of tools.

Having eliminated these to experiment with varying rates, styles and methods and find out the best.

(2) *By Scientific Methods.*—Having obtained an optimum method on practical lines, to employ scientific methods, e.g., estimation of energy, cost of work, motion study in its various forms, to test or improve its efficiency.

(3) *By Actual Practice.*—To train recruits in the new drill and test its efficiency by definite tasks or spells with special relation to the rate of output and the after-condition of the subjects employed.

(4) Tools used and Apparatus employed.

Tools.—Every experiment was conducted with both the R.E. pick and shovel and the G.S. pick and shovel. The following table gives a comparison of their weights and sizes :—

TABLE I.

Kind	Pick: Length	Pick: Weight	Shovel: Length	Shovel: Weight	Shovel: Size of pan
R.E.	36 in.	8 lb. and 4½ lb.	3 ft. 4 in.	5 lb.	12½ in. by 10 in.
G.S.	36 in.	8 lb. and 4½ lb.	3 ft. 2 in.	3½ lb.	9½ ,, by 8 1/16 ,,

Apparatus Employed during the Investigation.

(1) For the practical experiments platforms were constructed and also boxes of varying capacity—4 cubic feet, 8 cubic feet and 18 cubic feet, thus facilitating measurement of output. A chess framework to resemble the side of a trench, the height being adjustable up to nine feet, was also constructed.

All time measurements were made by a stop-watch, and when rate of stroke came up for investigation a metronome was used.

(2) In the cyclographic study of movement, a small electric globe was attached to either the tool or part of the worker's body, and by an interrupted current and the use of special photographic plates, graphic records of movements were obtained.

(3) In estimating energy expenditure the Douglas-Haldane method was employed, i.e., the expired air was collected in the Douglas bag and its analysis carried out on the small Haldane apparatus.

Care was taken to allow a definite time (about three hours) to elapse after the previous meal before any records were taken.

In the calculation of energy expenditure, the oxygen figures and the Zuntz values were employed.

Mean values were taken from the results of individual experiments.

(5) Conditions of Experiment.

(1) *Atmospheric.*—Conditions were standardized as much as possible though the weather was naturally variable. A certain number of experiments were carried out in a large open shed, conditions of weather and base being thus uniform.

(2) *Soil.*—A special piece of ground was chosen with soil of a more or less uniform type, and all experiments were carried out with this soil.

Type—medium; earth and gravel; weight per cubic foot, approximately eighty-five pounds.

Naturally slight variances were found in different parts of the ground, but on the average the weight per cubic foot did not vary to any extent.

(6) SUBJECTS OF EXPERIMENT (these numbered nine in all).

Personnel: One non-commissioned officer and six men. They were specially chosen to include varying types of physique and represented an average army squad. They had little previous experience in digging beyond the average soldier's work in this line, and their experience of training in the digging drill was very slight. Two skilled navvies were also employed for a short period; the authors of this report being unskilled in digging, served as useful experimental subjects.

SECTION I.

PRACTICAL OBSERVATIONS OF RELATIVELY SKILLED AND UNSKILLED WORKERS.

RECORD OF EXPERIMENTS.

The object of the observations under this section was to ascertain practically, by allowing each man to work in his own way (i.e., style, rate and rest-pause), the effect on output of varying methods, and to form a ***rough*** estimate of the optimum methods of digging with reference to length of spell, rate of pick stroke and shovel throw and length and time of rest-pause. Having obtained these records of what the various individual subjects could perform by their own methods, a basis for further research would be thus obtained.

All observations were carried out under similar conditions of soil and tools, and height and length of throw were standardized for all.

The standard height of throw aimed at was 4 feet 6 inches, and the horizontal throw 5 feet.

CONSIDERATION OF RESULTS.

Each individual subject's results were averaged and tabulated for varying spells of single and combined tasks, and from these a summary of averages for purpose of comparison between individuals and between types of tool was drawn up. The following two tables have been selected to show how these results were tabulated.

(*A*) *Picking.*

(1) *Length of Spell.*—In the members of the R.E. squad a short spell, i.e., under sixty seconds, appeared to produce better results than a long spell, but it was noticeable that in the case of the navvies this was not borne

out, each man having an average spell of about ninety seconds. This seemed to be due to two causes :—

(i) The inclusion of a raking stroke after the main pick stroke to clear the way for the next stroke.

TABLE II.—SUMMARIZED AVERAGES : R.E. TOOLS.

Subject	Average* total spell (secs.	Pick			Shovel			Rests		Output	Average shovel-load in lb.
		Average spell (secs.)	Per minute rate	Per cent raking	Average spell (secs.)	Per minute rate	Per cent scraping	Long† (secs.)	Change-over time (secs.)	Lb. per minute	
"D" ..	1,664	30·6	29·6	*Nil*	72·7	18·2	0·6	1,430	4·6	140·6	10·2
"B" ..	1,636	54·5	25·1	,,	93·1	13·6	2·1	30	4	115·6	9·6
"C" ..	1,609	61	32·2	,,	74·5	19	1·6	506	5·2	109	8·3
"G" ..	1,612	58·7	25·8	,,	71·2	19·9	1·3	570	4	101	6·0
"E" ..	1,624	80·1	26·8	,,	93·4	16·9	0·6	457	4·9	94·4	6·7
"F" ..	1,554	78·2	28·4	,,	98	13·4	1·3	1,165	4·8	81·8	7·7
Civilian labourer											
"K" ..	1,744	106·8	25·7	36	135·3	17·3	11·5	134	6·5	133·7	10·7
"L" ..	1,683	86·6	25·7	28	139·6	12·6	4·8	73	4·3	110·2	10·0

Note.—* Spells include periods of ten minutes to one and a half hours.
† Long rests include any rest over ten seconds, and, as shown, refers to the long spell only (one and a half hour's task).

TABLE III.—SUBJECT "K's" AVERAGES.

Task	Tools	Average total spell (secs.)	Pick			Shovel			Rests		Output	Average shovel-load in lb.
			Average spell (secs.)	Per minute rate	Per cent raking	Average spell (secs.)	Per minute rate	Per cent scraping	Long (secs.)	Change-over time (secs.)	Lb. per minute	
Shovelling	R.E. ..	415	..	..	..	..	19·1	..	..	..	221	11·6
	G.S. ..	504	..	..	..	..	22·4	*Nil*	..	..	182	7·2
	Dockyard	323	..	..	..	..	14·7	..	..	..	285	19·4
Picking ..	R.E. ..	120	..	27	33	..	..	..	..	..	269*	..
	G.S. ..	288	..	35·6	17	..	..	..	..	..	212	..
Combined (short)	R.E. ..	891	103·6	28	30	122	17·5	12	*Nil*	6	96·4	10·5
	G.S. ..	720	72	35·6	17	107	25·8	6·8	,,	4·3	85	6
	Dockyard	1,840	91·8	18·6	36	132	12·4	9·5	,,	5·6	102·5	15·9
Combined (long)	R.E. ..	5,550	110	22·1	45	148·6	15·2	11·6	134	7	83·8	10
	G.S. ..	5,400	97·1	27	35	138	21·2	6	180	4·3	76·5	6·1
Averages	R.E. ..	1,744	106 8	25·7	36	135·3	17·3	11·5	134	6·5	133·7*	10·7
	G.S. ..	1,728	84·6	32·7	23	122·5	23·1	6·4	180	4·3	114·5	6·4

* The output in pounds per minute as averaged does not include that of picking only ; the results obtained from this task, if included, would tend to give a wrong impression of the average output.

(ii) The better balance and stance of these men which enabled them to loosen and bring down more earth to the base without treading it in.

Neglecting the raking stroke the optimum length of spell on a 5 feet face by 2 feet deep would appear to be about 60 seconds with the R.E. pick, slightly longer with the G.S. pick.

(2) *Rate per Minute.*—This approximated closely in all men (neglecting short strokes), and the optimum rate would appear to be twenty-six to twenty-eight strokes per minute for the R.E. tool, slightly faster for the G.S. variety.

(3) *Methods.*—It was noticeable that with regard to the method of picking by allowing the tool to fall under its own weight, described in S.M.E. Fortification Circular, No. 61, the only subject who utilized this method was the worst performer as regards output, and that the skilled navvies made more use of their shoulder muscles and body weight in reinforcing their stroke than any member of the R.E. squad except the best performer (Subject D).

The raking method of the navvies as mentioned above appeared to be of value.

Balance and stance were very important factors, as also the motion of the forward hand in the downward stroke.

(*B*) *Shovelling only.*

(1) *Length of Spell.*—This depends so much on the length of the picking spell that any reduction or increase in the latter affects the shovelling spell; but in this type of soil, speaking generally, the length of period appeared to be about half again the time of the picking spell for the R.E. shovel, and for the G.S. shovel twice as long as the picking spell.

(2) *Rate per Minute.*—The optimum rate appeared to be eighteen throws per minute for the average man using the R.E. shovel. Marked increase or decrease on this rate would seem to diminish output. With the G.S. shovel the rate was about two extra throws per minute.

(3) *Methods.*—Balance and position of the feet appeared of the utmost importance.

It was noticed that the navvies made very much more use of their knees and body weight in assisting the forward thrust of the shovel blade into the material than the R.E. squad. This happened only with the R.E. shovel. No aids were used with the G.S. shovel.

Also the scraping factor was present in all though more pronounced in the navvies. This method of keeping a clean and smooth base though valuable can certainly be very much overdone, and if so, adversely affects output.

(4) *Shovel Load.*—In general, the greater the load the better the output. The optimum appeared to be about 10 lb. for the R.E. shovel, 6 to 7 lb. for the G.S.

Rest Pauses.—Changing over from one tool to another occupied on the average about five seconds, and can be regarded as a short rest during which time the subject straightens himself and eases any cramped position.

Long rests were almost invariably taken after a completed combined spell, i.e., at the end of the shovelling period after the loose earth had been thrown out of the trench.

The length of these rests varied considerably in different individuals. In 1½ hours spell with R.E. tools, periods from ½ minute total to 24 minutes total were taken, and it was a striking fact that the latter rest occurred in the performance of the best individual output.

The optimum rest period would therefore appear to be best taken at the end of a combined spell and would be a short period after a short spell, i.e., thirty seconds after every combined spell, or sixty seconds after every two combined spells. The latter would equal about twelve minutes per hour.

(C) General Observations on Section I.

These observations on the relatively skilled (R.E. squad) and the skilled (navvies) men have proved a most interesting and profitable study.

Methods, rate of stroke and throw, rest pauses, etc., voluntarily undertaken produced most variable and contrasting figures, yet it became obvious early on that good performers had essential points of similarity in their styles and rate of work as a contrast to bad performers who had the same obvious faults in technique.

Thus rhythm of movement in one or two performers was very faulty, a too rapid or too slow sequence of movements producing a jerky style and a poor result. Balance was thus affected, causing a great deal of shifting of the foot stance with corresponding inefficiency.

Another common fault was overswinging which also caused unsteadiness and resulted in a high degree of fatigue without a proportionate increase in output.

Tools were often put to an illegitimate use. Thus, earth insufficiently loosened by the pick was frequently scraped and cut into with the shovel in an endeavour to get it away, causing a proportionately high energy wastage.

Successive strokes of the pick often fell on the same spot and on material already broken up.

Occasionally so much earth was picked down that it was trodden underfoot and had to be loosened again prior to shovelling.

A rough and bumpy trench base was frequent chiefly due to insufficient pick work, and as a result much more energy was wasted in the shovelling period and the employment of "aids" was necessary.

These good or bad methods had a most marked effect on output of earth. Thus a well co-ordinated action in picking and shovelling performed in a rhythmical manner and at an easy natural rate would far exceed the output of a method which involved greater muscular effort and a quicker rate of work, performed in a badily co-ordinated manner.

Undoubtedly an optimum method, an optimum rate and an optimum

spell exist, and this optimum does not appear to differ much in different types of physique. This optimum method and rate coming in the natūral swing of the arms and shoulders tends to become rhythmical and as a result economizes energy and defers the onset of fatigue.

The best two performers were a contrast in physique: the one, subject "D" (R.E.), being short and light, the other, subject "K" (navvy), being tall and heavily built.

Their method and style on the whole approximated closely. Their output of earth and condition after work were easily in advance of any of the remainder of the experimental party.

Subject "D" during a long task rested for periods which, totalled up, more than doubled the combined rest periods of any other performer, yet his output was far in excess of the others.

With regard to the two types of tools used, the R.E. pick and shovel for general work and with all members of the squad (large and small) produced better results. Spells were shorter, rate of stroke slower and output was greater. The G.S. shovel in particular, for the amount of energy involved in using it, produces poor results. For an average soil its pan is too small, taking an average load of 6 lb. only, thus increasing the length of spell and by the relatively large number of throws necessary, tending to produce rapid fatigue.

The two picks showed less varying efficiency. Some men produced a better output with the lighter type (4½ lb. head), and in a certain type of soil (light) it would probably produce better results than the heavier pick (8 lb. head) and being lighter and more easily transported would "pay its own way"; but as an all-round tool for the average man and in heavier soil a pick-head of about 6 lb. would be more suitable.

(*To be continued.*)

NOTES ON THE SANDFLY GROUP, AND SOME OBSERVATIONS ON THE SANDFLY FEVER AT HINAIDI CANTONMENTS, 'IRAQ.

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Early in December, 1921, I left England for the purpose of making such observations as were possible on the life history and habits of the Sandfly Group, and also on Sandfly Fever in 'Iraq.

Basrah was reached on January 10, 1922, Bagdad on the 14th, and on January 16 I was posted to Hinaidi Cantonment.

This Cantonment is situated on the left bank of the Tigris, about six miles by road to the south of Bagdad.

Except for a small plantation of date palms and other trees, near the river, at the north-west corner of the Cantonment, there was nothing in the way of vegetation larger than "camel thorn," a small thorny plant rarely growing more than eighteen inches high. About April this year numerous small plantations of fruit trees were made in places where water was available. Small date palms and various types of eucalyptus trees were also planted on either side of the main roads; as to how these trees will grow remains to be seen.

The Cantonment measures about two miles each way at its widest part, and, previous to last year, the ground now built on was simple desert which, with the exception of a small number of sand hills, was flat and uninteresting as far as the eye could reach.

Buildings are now scattered about all over the area in a more or less irregular manner, the whole being divided into blocks, lettered from A to N. Each block is occupied by one or more units.

Various types of buildings have been erected from huge corrugated iron hangars for the large passenger-carrying aeroplanes, to matting structures used as latrines and bathhouses. Another type of building consists of empty kerosene tins plastered with mud; nearly all the stores, meat rooms, etc., of the new supply depot are of this type. Brick buildings of two types also exist, one, of burnt brick, now being put up for the Royal Air Force, and an older type, in which only pillars to support the roof girders are of burnt brick, the intervening spaces being covered in with sun-dried mud bricks, the whole being plastered over with mud. As may be readily imagined most of these buildings are ideal for the breeding of sandflies, as the walls, inside and out, show numerous cracks and crevices.

The roads are made of ordinary mud without any form of metalling, the result being that, when it rains, all motor traffic ceases, only mule roads, which run parallel to the main roads, being open for transport.

On each side of the main roads water channels have been dug to help drain the Cantonment, to convey water to the watering-places for animals and to provide water for watering the roads, to lay the dust in dry weather. There are in all about thirty-six miles of these channels, which are badly kept and overgrown in most places with weed. In the event of fire, water is pumped into the channels in the sector in which the fire has occurred by the E. and M. section, otherwise the channels are usually only half full.

High level water tanks with distributing pipes are now being put in, and it is hoped that when this work is completed, many of the open channels may be dispensed with.

Drinking water is derived from a separate pipe system, and is perfectly safe for drinking and washing purposes, though in the hot weather it is unpleasant to drink direct from the pipe—my bath was filled daily from the cold water tap in my bathroom and usually had a temperature of about 89° to 92°.

Surrounding the Cantonment on all sides is a high bund of brushwood and earth, intended firstly to keep out the river when it is in flood, and secondly for defensive purposes. On the bund at intervals of about 800 yards are block houses, and outside a double apron of barbed wire.

Looking from the top of the bund, Alwiyah (civil) Cantonment and some of the higher buildings in Bagdad can be seen to the north and north-west; Karradah, the area occupied by Brigade Headquarters and No. 61 Indian Stationary Hospital, can be seen amongst the palms to the west. A fringe of palm trees on the banks of the Diala river, about five miles out, with railway line to Kut-el-Amara, is seen to the south, whilst to the east is desert, extending out to the Diala river about four miles away. On this side also may be seen the old lines of Turkish trenches, used in the late war, for the final defence of Bagdad. Near the bridge, carrying the railway over the Diala river, to the south, may be seen the monument erected to the Lancashire Fusiliers killed in crossing the river.

Soon after arriving at Hinaidi Cantonment, endeavours were made to find sandflies or their breeding places, but although many Psychoididæ were found in January, no specimens of Phlebotomus (sandflies) were found until April.

The first specimen of the latter genus was seen, on a wall, on April 5, but this evaded capture; no more were seen until April 14, on which date a male or female (*P. papatacii*) was caught. After this the numbers increased rapidly, and I find it noted in my diary on May 7, "Sandflies descended in swarms, when the fan stopped, otherwise I would have had a good night's rest."

The flies appear to come out in batches, and it was possible to find them alternately in two buildings kept under observation; during one week one building would contain many and the other none, the next week this would be reversed.

The earliest species to be caught was *P. papatacii* on April 14; it was not until April 24 that the first specimen of *P. minutus* was taken, on a wall near an electric light. This latter species was not nearly so common as the former. These were the only two species caught during my stay at Hinaidi Cantonment, and although *P. sergenti* is reported as occurring in 'Iraq, no specimen was caught by me.

The family Psychoididæ belongs to the order Orthorrapha, suborder Orthorrapha Nematocera (long thread-like antennæ), and is divided into the following genera:

(*a*) Phlebotomus.
(*b*) Psychoda.
(*c*) Pericoma.
(*d*) Trichomyia.
(*e*) Sycorax.

Of these Phlebotomus is the only genus which concerns this article.

The Genus Phlebotomus.

These are small Diptera, with the wings and bodies thickly covered with hairs. *P. papatacii* is a large species, whereas *P. minutus*, as its name implies, is very small, so small in fact that it is almost impossible to see pinned specimens without the aid of a hand lens.

The wings, which are described as oval or lanceolate, when at rest are held at an angle of about 45° with the body in this species, whereas in the other species of the family they are folded down in a manner similar to that commonly affected by moths. This method of holding the wings is of great assistance in picking out specimens of Phlebotomus from a large collection of Diptera round lights and on walls. The wings have a peculiar venation of their own and to a casual observer appear to have nine or ten longitudinal veins, which are nearly parallel, with no transverse veins. This is due to the facts that the veins usually branch nearer to the root than the tip of the wing, and that the transverse veins are very small and inconspicuous, and are also situated near the root of the wing, and that the second longitudinal vein has three branches, all of which are conspicuous, especially in cleared specimens.

The presence of hairs on the wings also tends to mask the venation to a considerable degree.

The distinguishing feature in the wings of this genus is that there are two simple longitudinal veins between the forked veins and that the first simple vein arises from the forked vein much beyond the anterior cross vein (Williston).

The eyes are large and black, clypeus prominent, palpi long and flexible, proboscis of moderate length.

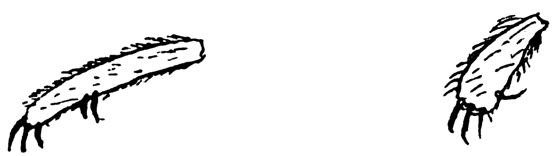
The antennæ are long and bead-like, consisting of sixteen segments, of which the first is much longer than the remainder and the three terminal are more beaded.

The legs are long, the tibiæ have no spines, claws are simple.

Movements of the fly are rapid, and when attempts are made to catch specimens in tubes from off walls, they make a short flight of an inch or two, which resembles a jump, and after two or three of these short flights usually fly away. Owing to the rapidity with which they make these small flights they are extremely difficult to catch in tubes, but may be more easily caught if an electric fan is turned on, as they then appear to be afraid to release their hold of the wall. The range of flight was not fully worked out, but specimens were watched flying the length of two rooms, totalling about forty-five feet, and certainly seemed to tire towards the end of the flight and could be caught on the wall at the end quite easily. Probably about fifteen to twenty yards would be the extreme range of flight for normal specimens, gorged females flying a lesser distance,

The distinguishing of the various species is difficult unless males are caught; the wing venation differs slightly in all species; type specimens of each species are required for comparison. In the males, however, the claspers differ to a considerable degree in each species.

The best way to deal with the specimen is to soak it for a short time in weak caustic soda, to dissolve away as much of the soft tissues as possible. The fly is then washed, stained, dehydrated, cleared and mounted in the usual way and then examined under a low-power, about one inch to two-third inch objective.



P. papatacii. *P. minutus.*

Terminal Male Clasper.

In *P. papatacii* the terminal segment of the male clasper carries three terminal spines and two spines at about the junction of the middle and terminal thirds of the clasper, the latter being on the inner side of the curve.

In *P. minutus* there are three terminal and one sub-terminal spine.

Owing to pressure of other work it was not possible to devote one's time entirely to the study of the habits of these flies, but certain points were noted.

Adults were found in large numbers in damp rooms, such as wash-houses, sculleries and bathrooms, but also invade bedrooms, especially those which were kept darkened in the day time, where they hide behind any clothing hanging on the walls and behind boxes and cupboards.

Females only were found to bite, chiefly at night, but one is frequently bitten by day if wearing "shorts" whilst sitting at a writing-table.

If kept in tubes, containing damp blotting paper, females will feed at any hour of the day. When feeding, the fly occasionally rests and moves

away to a fresh spot and then resumes its meal—several, fed on my forearm, were seen to make at least three such changes, even though they had not been disturbed in any way. As soon as a full meal has been taken the fly endeavours to find a darkened spot in which to rest.

The slightest wind seems to prevent the flies coming out, so that they are most troublesome on still nights. An electric fan will keep them away, but should the fan stop the last state is worse than the first, as they seem to arrive in hundreds, get into the bed and remain there.

Both sexes are attracted by artificial light—the first pair found *in copulo* was taken from a wall within a few inches of an electric light.

During the day they usually endeavour to find some form of cover, such as hanging clothing and rugs, on walls and other such places—coming out at night to feed.

They are able to reach a considerable distance from the ground, being just as troublesome on still nights on the roof as inside buildings. Officers sleeping on the roof at No. 23 British Stationary Hospital, a distance of nearly forty feet from the ground, frequently complained of being bitten by sandflies.

Both males and females are able to get through nets having a mesh of twenty-two to the inch, they also get into the bed from underneath, working along between the layers of the bedding. I have frequently seen them entering my bed by getting under the top end of the upper sheet; once inside the bed they attack the arms and ankles or any of the exposed parts. (More will be said about the habits of adults when considering sandfly fever in Part II of this article.)

Eggs.—Only one batch of about forty eggs was obtained: these were laid by a female, previously fed on my forearm, on wet blotting paper in a test tube. Unfortunately I contracted sandfly fever at the time the eggs were laid, about three days after the blood feed had been given, and by the time I felt fit enough to take interest in entomological matters the eggs had partly dried and did not hatch.

The eggs were very elongate, large for the size of the fly, dark brown in colour and covered with a raised network of darker wavy lines.

No larvæ were found, although many likely breeding-places were examined and much rubbish and debris from various sources were placed in tubes and tins. No pupæ were found.

PART II.

SOME NOTES REGARDING SANDFLY FEVER.

Sandfly fever was very prevalent during May, June, and July. The first case was reported on May 1, and up to the end of July 287 cases amongst British troops alone had been treated at the Post Hospital, exclusive of cases which had been sent into No. 23 British Stationary Hospital, Bagdad. At the latter hospital the number of admissions from various

sources for sandfly fever were: 302 for May, 414 for June and 443 for July. Except in special cases no patient was sent in from Hinaidi, on account of this disease, until his temperature was over 103°.

As stated previously the first sandfly was seen on April 5, but it was not until about a month later that the first case of sandfly fever came for treatment.

The symptoms of the disease are very similar to those of influenza at home, but there are no catarrhal symptoms. One very constant symptom is pain behind the eyes with congestion of the conjunctivæ.

Perhaps a description of a personal experience, which appears to be more or less normal in its course, may prove of interest. For personal information I kept up a case sheet of my symptoms.

On April 28 some sandflies, confined in a test tube, were fed on my forearm, in order that, if possible, some eggs might be obtained.

Previous to this I had been badly bitten on April 27. On May 1 I felt rather seedy, there were no local symptoms of which I could complain, but I felt a general condition of malaise; smoking became unpleasant, always a bad sign in my case. May 2: Still feeling seedy, with myalgic pains all over, especially marked in the back and neck; headache and pain behind the eyes. Tongue clean. bowels open, skin dry, pulse normal.

I had finished my office work by 10 a.m. and lay down until lunch time 1 p.m.; owing to a feeling of nausea I did not eat any lunch, and went back immediately afterwards to my room and again lay down. By this time the headache was intense, eyes very painful and congested, legs weak. At 4 p.m. feeling worse I decided to undress and go to bed; there was nausea and attempts at vomiting. Pulse 80; temperature 103° F.

I had no dinner, took ten grains of quinine bihydrochloride and slept well.

Blood slides were found to be negative to malaria.

May 3: Morning temperature normal, headache better, general feeling of weakness. Bowels not open; pulse 64; this pulse-rate was constant throughout the day. Evening temperature 101·6° F.; pulse 64; pains in back, both iliac fossæ and over the liver, the latter rather severe.

May 4: Morning temperature normal, still feeling very weak, but as there was a man in the guard detention room who had to be dealt with, I had to get up, only to find that my legs were very weak and practically useless, so I had to go over to the Office, a distance of about 150 yards, in an ambulance. Appetite bad; bowels open; medicine as before. Temperature normal; pulse 80.

From this date onward I gradually recovered, but had a feeling of weakness for about a week afterwards. There were no complications.

These symptoms of mine were, I think, fairly typical and identical with many others seen in the wards—in some cases, however, there is not the slowing of the pulse. The extreme weakness after the attack is a very constant feature, and men when sent out after an attack are always given a few days off duty in which to regain their strength.

Only one case of jaundice was noted amongst all the cases I have seen, which ran into some hundreds, and in this case it is doubtful whether his original diagnosis of sandfly fever was correct.

Diagnosis.

At first there were no means for examining blood slides at Hinaidi, but later on a supply of slides and Leishman's stain was obtained. The blood of every fever case was then examined by Captain Tibbs, R.A.M.C.; in only one case was anything abnormal found, namely, B.T. parasites in a case of malaria.

The diseases from which sandfly fever has to be differentiated are: Malaria, dengue, typhus, the paratyphoids, and influenza.

Malaria.—The past history of the patient and the absence of parasites in the blood helped to eliminate this disease.

There were no fresh infections of malaria at Hinaidi, as luckily anophelines were rare; although careful search was made only four were caught during the whole of the time (six months) I was in the Cantonment. All four belonged to the same species—*A. pulcherrimus*.

One of these behaved in a manner quite contrary to the usual habits of mosquitoes and suffered death accordingly. It bit an officer in the latrine at 2·30 p.m.; the temperature at the time, in the latrine, was 113° F. The latrine was a simple corrugated-iron structure—with many large openings and quite as well lighted as the mess verandahs.

Dengue.—The vectors—*Ædes calopus* and *Culex fatigans* (?)—were not found in the Cantonments, nor did any of the patients develop any skin rash.

Typhus Fever.—Lice may be said to be entirely absent among the British other ranks, though one or two men arriving with fresh drafts occasionally were found to be infested. Amongst Indian other ranks lice were in many cases common in spite of disinfestation. This disease could be dismissed owing to the short duration and absence of rash, also on account of there having been no deaths.

Paratyphoid Fevers.—Although it was not possible to carry out agglutination tests, the short course of the disease eliminated this group.

Influenza.—There is little doubt that had these cases occurred in England they would all have been diagnosed as influenza—but the absence of catarrhal symptoms and the slowing of the pulse in cases of sandfly fever are of use.

There is no doubt that there is still a large field for further investigation of this disease. Regarding the method of infection—this is undoubtedly due to the direct bite of the sandfly. It has been suggested that the infection may be carried from one generation to the next through the ova, as in the case of yellow fever (Hindle)—in favour of this is the fact that all the flies fed on my arm were unfed females, and although others had bitten me on the previous night, not any of those caught showed any signs of having taken

a meal previously. No other cases had been reported in the Cantonment—the date on which the symptoms first showed in my case coincided with the date of the first case to be admitted from other units, namely, May 1; no one living on the Mess premises or in any adjacent building, the nearest of which was over fifty yards away and so probably well beyond the range of flight of sandflies, had been ill. Even on still sultry nights, under the most favourable conditions, it is doubtful whether sandflies can fly more than fifteen to twenty yards.

Intermediate hosts have been suggested by some as being the reservoirs of the disease, such as lizards, frogs and other reptiles. One young lizard was found in the bathroom late in June—frogs were found in the water channels on the roadsides, the nearest being over 100 yards from the Mess, and it was not until the end of May, after a garden had been commenced, that toads were seen anywhere near the Mess. In any case, had these flies previously fed on any living creature, some indication of the meal would probably have been seen in the abdomen. So far I have not seen any report that the stomach contents of sandflies have been found to contain reptilian blood.

The prevention of sandfly infection is one of the most important duties of all medical officers doing duty in the tropics, and although certain individuals have paid much attention to this, no general work seems to have been done.

Several methods were tried at Hinaidi Cantonment, but none gave very satisfactory results.

The following were tried:—

(*a*) Spraying of walls of all buildings to a height of ten feet, also spraying of rubbish heaps. This was done at least twice a week, more often if possible, with either cresol solution, an emulsion of crude oil, or Army fly spray. This method probably destroyed many flies and larvæ, but was not sufficient to prevent the disease.

(*b*) *Nets.*—These were issued to the men, but only a small percentage were given the "22 mesh"; these were found to be unbearably hot on still nights and men could not be induced to make proper use of them. From experiments carried out it was found that *P. minutus* immediately escaped from tubes covered with the "22 mesh" netting and *P. papatacii* with some difficulty—fed females seemed unable to escape—but this is of little consolation as by the time the female has fed the damage has probably been done.

The method employed in these experiments was to place flies in a dry test tube, over the end of which a piece of netting was tied; a second tube containing damp blotting paper was then tied over the mouth of the first tube, the netting naturally being between the two tubes. If the flies wanted moisture they had to get through the netting. *P. minutus* usually escaped before the second tube could be placed in position, and in only one case did *P. papatacii* fail to get through in the course of the night.

The flies also appear to work their way into the bed from underneath, between the tucked-in net and the bedding.

It would be impossible in the hot weather to sleep under nets of a smaller mesh than 22, owing to lack of ventilation.

(*c*) Sleeping outside and on the roofs of buildings: On still nights the flies were just as troublesome outside as inside; on windy nights there were no flies, but there was always the chance of a dust storm which would drive everyone into the buildings.

(*d*) Electric fans if in sufficient numbers were found to be good, providing the current was constant; if the fans stopped the sandflies came down in swarms and got into the beds, where they continued to feed throughout the night.

At one time the fans were frequently stopping, owing to the current having to be brought from Bagdad, but later a local power station was built and the supply of current considerably improved.

The number of fans supplied to the barrack-room was insufficient, 3 to a room of 22 men and 5 to a 40-bedded room—in the new barracks there has been a large increase in the number of fans supplied.

(*e*) Repellants: Vermijelli was tried by some; but owing to the mess it made of bedding and clothing was not popular. Citronella and paraquit were also used, but the effects usually wore off in three or four hours, so unless one was prepared to sit up and re-apply these, the preparations were useless—as a temporary measure paraquit was found to be the most efficacious.

It is fully realized that these notes are by no means perfect, but considerable attention was given to the subject, and it is hoped that they may help others serving in sandfly infested districts in any further investigations they may carry out in their endeavours to combat the disease. If so the writer's efforts have not been in vain.

NOTE ON A MODIFICATION OF THE SACHS GEORGI REACTION FOR SYPHILIS.

By Major A. DAWSON.
Royal Army Medical Corps.
AND
Assistant Surgeon R. O. A. SMITH, I.M.D.

When working in the Central Dermatological Laboratory on the original Sachs Georgi flocculation test [1], we found that a large number of obvious syphilis cases, giving positive Wassermann results, gave negative Sachs Georgi results.

These discrepancies may be explained by the presence of inhibition zones, and Dreyer and Ward [2] in their flocculation test endeavour to obviate these by putting up each serum in nine dilutions.

However, in practice we found the Dreyer-Ward method, although giving good results, to be a very laborious test when large numbers of sera have to be dealt with, also a special apparatus is required.

In the method now described we put up three dilutions of each serum and the simple pipettes used can be prepared in any laboratory.

Description of the new Modification.

The extract is prepared as follows :—

Twenty grammes of calf's heart muscle, free from fat, are finely minced, and extracted with 100 cubic centimetres alcohol at room temperature for forty-eight hours. The extract is then filtered through fat-free filter paper, and placed in the ice-chest for three to four days, by which time a fine sediment has precipitated.

For use, 10 cubic centimetres of the above extract are added to 20 cubic centimetres of alcohol, and 1·35 cubic centimetres of a one per cent solution of cholesterin in alcohol are also added. This mixture can be kept as a stock solution in the ice-box.

Immediately before use one part of the above mixture is slowly mixed with an equal part of 0·85 per cent normal saline, then four parts of saline are added rapidly.

Patient's Serum is collected as for the Wassermann test, and before use is heated for one hour at 55° C.

Technique.—Three tubes are used for each specimen of serum to be tested.

The tubes should be of fine clear glass, with an internal diameter of not more than 7 millimetres, and about 7·5 centimetres long, i.e., ¼ inch by

3 inches. These tubes are arranged in sets of three in Wassermann trays, one space being left at the end of each three for the serum tube.

To No. 1 tube are added 0·2 cubic centimetre serum, 0·8 cubic centimetre normal saline, 0·5 cubic centimetre diluted extract.

To No. 2 tube are added 0·1 cubic centimetre serum, 0·9 cubic centimetre normal saline, 0·5 cubic centimetre diluted extract.

To No. 3 tube are added 0·05 cubic centimetre serum, 0·95 cubic centimetre normal saline, 0·5 cubic centimetre diluted extract.

Method of making the Dilution and adding the Extract.

Three pipettes are required, one marked at 0·05 and 0·1 cubic centimetre, one marked at 0·5 cubic centimetre, and the third at 0·9 cubic centimetre.

To each test tube add 0·9 cubic centimetre sterile normal saline, and from tube No. 1 take 0·1 cubic centimetre, and of this add 0·05 cubic centimetre to tube No. 3. Thus one has in tube No. 1, 0·8 cubic centimetre saline, in tube No. 2, 0·9 cubic centimetre, and in tube No. 3, 0·95 cubic centimetre saline. So, when the serum and extract have been added the final dilutions of serum are: in tube No. 1, 1 in 7·5, in tube No. 2, 1 in 15, and in tube No. 3, 1 in 30.

The 0·5 cubic centimetre pipette is used for adding the diluted extract.

When the extract has been added, each tube is closed with the finger, and inverted slowly to mix the contents.

The trays are then placed in a water bath at 37° C., and are kept there for four hours, at the end of which time they are removed and a preliminary reading can be made. The trays are then placed on the bench and kept at room temperature for twenty hours. The final reading is made at the end of twenty-four hours.

In recording the readings the terms used by Dreyer and Ward [2] are employed and, to facilitate reading, the lamp previously described by us is used [1].

Differences between this Modification and the Original Sachs Georgi Test.

Extract.—Calf heart is used instead of bullock heart, as we find this gives a better extract.

Patient's Serum.—We heat the serum for one hour at 55° C. instead of thirty minutes as recommended by Sachs Georgi, to help in avoiding inhibition action.

Dilutions of Serum.—We put up dilutions of 1/7·5, 1/15 and 1/30. By doing this we find that we avoid much of inhibition action which occurs with a single dilution of 1/15.

Incubation.—We incubate in a water bath in order to get the advantage of convection currents. We incubate for four hours instead of two, as we find that more marked results are obtained.

RESULTS OBTAINED BY THIS MODIFICATION.

	Agree						Disagree				
	Positive	Negative	Partial	W ± S +	W + S ±	Total	W + S −	W − S +	W − S ±	W ± S −	Total
S_1A	103	8	3	3	1	118	5	1	0	0	6
S_1P	6	36	2	3	0	47	1	1	2	2	6
S_2A	97	0	0	0	3	100	4	0	0	0	4
S_2P	51	15	9	8	0	83	2	0	0	1	3
S_3A	8	0	0	0	1	9	0	0	0	0	0
S_3P	2	1	0	0	0	3	0	0	0	0	0
Latent_A	1	0	0	0	1	2	0	0	0	0	0
Latent_P	2	0	0	0	0	2	0	0	0	0	0
Non-venereal	1 (Malaria)	30	1	0	0	32	1	2	0	0	3
Venereal sores	0	68	5	19	0	92	0	3	0	3	6
Congenital	2	0	0	0	0	2	0	0	0	0	0
No history	4	4	1	0	0	9	0	0	0	0	0
	277	162	21	33	6	499	13	7	2	6	28
	Agree: 499 · 94·6 per cent						Disagree: 28. 5·4 per cent				Total 527

$S_1 S_2 S_3$ = Syphilis: Primary, secondary and tertiary.
A = Before treatment.
P = After treatment.

So far we have tested 527 sera, and have found 94·6 per cent to agree with the Wassermann tests on these sera. In the previous series in which we tested 246 sera with the Wassermann test, and also with the original Sachs Georgi test, we found only 85·4 per cent of agreement, which is as high a percentage as most observers claim :—

		Agreement	
Merzweiler (*Deutsch. mediz. Woch.*, November 13, 1919; *Brit. Med. Journ.*, Epitome, January 24, 1920)		86	per cent
Messerschmidt (*Deutsch. mediz. Woch.*, February 5, 1920)		85	,,
Okell (*Lancet*, November 5, 1921)		90	,,
Large and Wood (JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, March, 1922)—			
	In 24 hours	81·6	,,
	In 48 hours	83·5	,,
Webb (JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, August, 1922)		74	,,

Of the 527 sera tested by our modification we found that in forty-one there was no readable reaction in the 1/15 dilution, the original Sachs Georgi dilution, while either the 1/7·5 or the 1/30 gave a readable reaction. So with a dilution of 1/15 there would have been only eighty-seven per cent of agreement, thus showing the advantage of the two additional dilutions.

[1] DAWSON and SMITH. JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, February, 1923.
[2] DREYER and WARD. *Lancet*, May 7, 1921, p. 956.

Clinical and other Notes.

A CASE OF CHRONIC PARENCHYMATOUS NEPHRITIS TREATED BY DECAPSULATION OF THE KIDNEYS.

By Majors J. H. SPENCER and J. M. WEDDELL.
Royal Army Medical Corps.

Bandsman ———, aged 23, admitted to Queen Alexandra Military Hospital on December 30, 1922, for chronic parenchymatous nephritis. Condition on admission: Œdema of face and ankles. Heart normal. Blood urea thirty-nine milligrammes per 100 cubic centimetres. Urea concentration test 2·8 per cent. Urine albumen 2·5 per cent. No casts. Chlorides 0·53 per cent.

Patient's condition became steadily worse in spite of treatment on a salt-free Epstein's high protein diet combined with purgation and the exhibition of urea and caffeine.

By January 20, 1923, the condition was one of severe general anasarca. The urine had fallen in quantity to twenty-one ounces in twenty-four hours. During the ensuing week energetic treatment directed towards reducing the anasarca was adopted and consisted of half-ounce doses of urea twice daily with free purgation and hot-air baths combined with pilocarpine injections. Under this treatment the quantity of urine rose to thirty-one ounces per twenty-four hours, but soon fell again to twenty to twenty-five ounces.

Dr. Herbert French saw the case on January 22, 1923, and advised decapsulation of the kidneys. The cardiac condition was throughout satisfactory.

February 2, 1923. Operation: Colonel West and Major Weddell. General anæsthesia—chloroform.

The right kidney was exposed through the usual lumbar incision. The tissues were very œdematous and also the perinephric capsule, which was distended and looked like the ascending colon. Considerable difficulty was experienced in delivering the kidney which was enlarged. A cyst about two inches by two inches was present at the lower pole of the kidney; this was punctured and clear fluid escaped. The capsule which was thin and transparent was incised longitudinally along the convex border of the kidney and easily stripped down to the region of the hilum. The capsule was not cut away. The kidney was replaced in the loin and a drainage tube inserted to the lower angle of the wound. The wound was closed in layers. Hæmorrhage was very slight.

A similar procedure was adopted on the left side, the left kidney delivered

and the capsule incised and stripped as before. A drainage tube was inserted and the wound closed in layers.

The condition of the patient at completion of the operation was very satisfactory.

February 3. Had a good night and passed thirty-one ounces of urine, during the twenty-four hours after operation. Face slightly less œdematous.

February 7. Irregular temperature since operation. Feels comfortable. Passing about thirty ounces of urine daily. Put on infusion of digitalis two drams three times a day.

February 9. Quantity of urine increasing up to sixty ounces in the twenty-four hours.

February 15. Urea, two drams, and caffeine cit., five grains, daily. Digitalis omitted.

February 24. Quantity of urine very much increased, from 132 to 202 ounces in the twenty-four hours. Anasarca very much less.

February 27. Urea reduced to one dram daily.

Patient made gradual improvement, and on March 1, 1923, the urea was reduced to thirty grains daily. On March 10 the urea was discontinued.

Patient allowed up; there is no œdema of ankles.

March 15. Patient is now up half a day and there is no anasarca present. Meat has been added to the diet. Urine contains albumen varying from 0·5 to 0·1 per cent. He is taking a mixture of tr. digitalis five minims and tr. ferri perchlor. ten minims, three times a day.

The immediate result of the operation was that a large amount of œdematous fluid was poured out from both drainage tubes, apparently gravity drainage from the most dependent portions of the body. The next œdema to disappear was from the face, and the last to clear up was from the ankles and legs. There was no special difficulty in the operation itself, the most difficult part was in delivering the kidneys out of the wound. The tissues were full of fluid and were difficult to recognize. The stripping of the kidney capsules was easily carried out. No part of the capsule was actually cut away.

The *rationale* of this operation appears to be that fresh vascular anastomoses are formed between the raw surfaces of the kidneys and the perinephric capsule, and the operation is usually recommended for cases of chronic nephritis. For this case a great part of the relief would appear to be due to the mechanical drainage of the tissues, which allowed the kidneys to recover. A very marked increase in the amount of urine was seen on the exhibition of urea after the operation, although it had been administered in large doses before operation with very little benefit.

MERCURIAL STOMATITIS.

By Captain J. McLEAN FOREMAN.
Army Dental Corps.

Mercurial stomatitis, although rarely seen in private practice, is met with daily in the Dental Department of a Venereal Hospital, and as Dental Surgeon in charge of such a department, I have been afforded every opportunity of studying the condition, common to almost every patient undergoing treatment for syphilis.

After many attempts with varying degrees of success, I have at last adopted a method of treatment which is speedy and gives excellent results both after and during the time the patient is having his injections.

It is of the utmost importance that patients be kept under strict observation twice weekly, immediately on admission to hospital and throughout the course of treatment, so that preventive treatment may be carried out and every opportunity afforded to check the first signs of stomatitis.

Preventive Measures.

The following means may be adopted to prevent as far as possible the stomatitis becoming severe.

The results to a great extent depend on the assistance rendered by the patient himself during treatment, and a five minutes lecture on oral hygiene to patients on admission is extremely helpful.

The after-effects of the injections can almost in every case be gauged by the condition of the patient's mouth on the commencement of treatment.

At least 75 per cent of the severe cases of mercurial stomatitis are due to neglect of the mouth before admission to hospital. This could be easily remedied if Platoon Commanders had a daily mouth inspection to see that tooth brushes had been used. All patients should be inspected as early as possible after admission and the mouth thoroughly cleaned.

A peroxide of hydrogen mouthwash should be given (one in one) to be used after meals. Every time he attends for treatment the patient should bring his tooth brush for disinfection, so that the danger from re-infection from that source may be eliminated.

Dentures should also be disinfected regularly, and in cases of oral neglect the dentures should be temporarily confiscated by the Dental Officer until the mouth is again healthy and the patient taking an interest and helping in curing his disease. Treatment is very often delayed owing to patients being sent into hospital without small kit (toothbrush), and the most thorough treatment is of no avail if the patient fails to keep his mouth clean. If units could take steps to prevent this happening much labour would be saved and a great service rendered.

If the above measures are adopted with a normally healthy mouth from the commencement of treatment there is little fear, except in isolated cases, of the mercurial stomatitis becoming severe.

The worst offenders are out-patients who fail to report at the Dental Department on the days they attend hospital for injections and carry on until compelled to give in, by pain and inability to masticate ordinary diet.

I shall first deal with the treatment of a patient who has had his mouth "cleaned" and who takes a moderate interest in oral hygiene.

(1) In the majority of cases the patients complain after the second or third injection of tenderness of the palate, just behind the incisors. Very often at this stage very little can be seen to account for the tenderness. Sometimes the gums may be a little red or a slight puffiness observed.

Treatment.—This refers to cases where scaling has already been carried out.

Cover the point of a fine bristle with cotton wool, saturate with chromic acid (100 per cent) and apply carefully round the necks of the teeth, on the lingual and buccal aspects.

On careful observation it will be noticed that in many cases a minute bead of pus has been expelled at the cervical margin due to the visible contraction of the gum tissue.

A mouthwash of hydrogen peroxide and water—equal parts—immediately after the application of the chromic acid, prevents any severe caustic effects. This mouthwash should also be used after every meal.

After the first treatment the tenderness completely disappears. The continued success depends entirely upon the regular twice-weekly inspection and necessary treatment, and it is only in rare cases that a severe stomatitis develops.

(2) Advanced cases of stomatitis arise when patients have been confined to bed and neglected to brush their teeth, or failed to keep their appointments. On examination, the breath is extremely fœtid, and there is marked ulceration of the gums, especially in the region of the incisors and third molar. It is quite common for the patient to state that he has been unable to eat food for two or three days, and that the pain kept him awake during the night. The toothbrush has already been out of use for a week or two and the necks of the teeth are crusted with a deposit of tartar, pus cells and decayed foodstuffs. The pain is so acute that scaling is unbearable.

Treatment.—Syringe with hydrogen peroxide and water—warm—between the teeth and remove the loose matter. Then go carefully round with cotton wool—using pellets and tweezers—and remove as much as possible of the tartar, etc. Clean well between the teeth and remove matter loosened by the syringing. Often a raw bleeding surface is left. After the patient has washed his mouth out well with peroxide and water

swab the gum margins with chromic acid (100 per cent), using the pellets of wool and tweezers, and again use of hydrogen peroxide.

The concentrated solution of the chromic acid caused considerable discomfort for an hour or two after treatment, but in all cases the patients declared that the apparently drastic treatment was worth while, and that their sleep was not disturbed. The peroxide mouthwash is used after every meal and is found very beneficial. I have found without exception that the patients on milk diet recover much quicker than those on ordinary rations. Five to seven days milk diet is usually sufficient; as it is difficult to ensure "out-patients" getting milk diet in barracks, it is recommended to admit severe cases to hospital until the condition clears up.

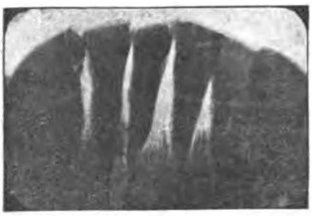
On the second examination two days later, it is found that the teeth can be scaled without unusual pain. Chromic acid is again applied as before. In cases where considerable absorption of the alveolar processes and recession of the gums has taken place, massage is extremely helpful in restoring the normal circulation and hardening the gums.

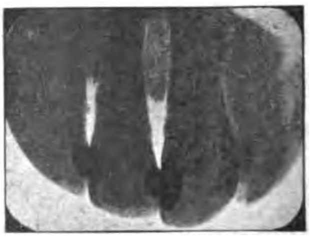
Unless absolutely essential, the removal of teeth whilst the patient is undergoing his course of injections should be avoided. Extraction at such times has been followed by extensive sloughing and later absorption, causing great difficulty in cases where dentures are required.

It is advisable for patients wearing part dentures to leave them out altogether until the mouth has quite recovered; dentures, when worn, should be disinfected daily.

Toothbrushes also are a continual source of re-infection, and they should also be disinfected each time the patient attends for treatment.

Injections of intramine without local treatment appear to produce very little change in the mouth, although local treatment without intramine is highly satisfactory.





After much difficulty, I got one or two X-ray photographs of the incisor regions of the patients undergoing treatment for syphilis. "A" who reported when his mouth was extremely bad, has had one and a half full courses of injections. The photographs show marked absorption.

I hope to get a series of photographs taken before, during and after treatment which will show the wonderful results effected by the above line of treatment of mercurial stomatitis.

The photo on the left shows marked absorption. The patient had two full courses of injections for syphilis and had a very bad mouth when he reported for treatment. A remarkable recovery was effected with massage and the usual treatment.

The photo on the right is that of a patient undergoing treatment for syphilis; he recently reported complaining of pain over region of 5. This root with abscess sac complete, was removed painlessly under local anæsthetic.

A PRELIMINARY NOTE ON THE VALUE OF INTRAVENOUS INJECTIONS OF ACRIFLAVINE IN THE TREATMENT OF GONORRHŒA.

By Captain G. H. WOOD.
Royal Army Medical Corps.

Having recently had an opportunity of testing this method of treatment in King George V Hospital, Dublin, a brief account of it may be of interest.

The ordinary treatment of gonorrhœa in the Army is disappointing in its results, and a very large number of days of inefficiency is caused by this disease; any treatment which would appreciably shorten the period of days in hospital was accordingly welcomed, and every effort made to test the value of this method of treatment and appraise it at its true worth. Theoretically the intravenous injection of acriflavine appeared to be an eminently rational procedure, as apart from its action on the blood-stream which has not yet been worked out, it promised to promote an increased flow of bland, non-irritating, antiseptic urine, which would be distinctly inimical to all gonococci within its reach.

It would thus provide an excellent method of washing out the urethra from within, along its entire length, from the very start of the disease, before the gonococci had time or opportunity to burrow down to the deeper tissues. It is well known how difficult it is to irrigate the entire urethra during the first three or four days of an acute gonorrhœa and how likely it is, that while washing out the anterior urethra some infective matter may very easily get past the barrier of the compressor urethra muscle, and, not being flushed out again, may cause an extension of the disease into the posterior urethra.

Colonel Harrison rightly regards acute gonorrhœa as a surgical condition requiring constant and easy drainage, and accordingly advocates the use of

urethro-vesical irrigations from the commencement of the disease, a weak non-irritating irrigating fluid such as potassium permanganate 1—10,000 being usually recommended. He will readily admit the difficulty of posterior irrigations with an acutely inflamed urethra, and in any case this needs the attention and skill of a well trained attendant. In these circumstances it appeared to be feasible to permit the patient to wash out his own urethra by means of the contractions of his vesical muscles, and as a further means to this end, the flow of urine was ncreased by a plentiful milk diet supplemented by two pints of barley water daily. To render the urine non-acid and non-irritating, an alkaline mixture containing potassium bicarbonate, potassium citrate, and tincture of hyoscyamus, was given three times daily. In private practice this could be substituted by four-hourly ten-grain powders of potassium acetate.

In actual practice the effect of these injections of acriflavine solution was distinctly encouraging; it was observed that there was a marked diminution of the urethral discharge, and the distressing "chordee" and painful micturition rapidly vanished, an important advantage especially in private practice.

It was, however, discovered that in some cases where after three injections during a period of about ten days, the symptoms of gonorrhœa had almost disappeared, i.e., complete cessation of urethritis, if treatment was not persisted in the discharge recurred when the patient was put on ordinary diet and allowed to resume his ordinary routine. In these cases another injection, or continuing the daily astringent irrigation for some days after the discharge has ceased, made all the difference between real and apparent cure, and proved that it was unwise to stop the treatment too soon.

In other cases the symptoms of gonorrhœa persisted in spite of repeated injections, and proved the necessity of combining this treatment with other recognized methods.

Contrary to what one might have expected these injections were tried with successful results in chronic cases which had been lingering on under ordinary irrigation treatment; in these marked improvement followed a couple of injections even when the patients had already been under treatment for a couple of months. In all cases where the discharge had practically ceased, but the occurrence of threads and filaments in the urine persisted, the action of the drug was helped by dilatation and massage over straight sounds with a view to breaking down submucous infiltrations, and opening up the various pockets where the gonococcus is likely to be sealed in beyond the reach of irrigating fluids. In certain cases, also, massage of the prostate followed by further injections was very helpful; in these cases the prostate apparently absorbed some of the acriflavine from the urine as it was being voided, and so the object of the injections was obtained, namely, to enable the drug to get in contact with the gonococcus right along the whole length of the urethral canal, from the commencement of the disease until the patient could be pronounced cured.

For the most part the patients were kept in hospital fourteen days after the cessation of all symptoms and the urine had become clear, three smears which contained neither pus cells nor micro-organisms being obtained in each case before discharge.

The object of this paper is not to claim any remarkable success for this method of treatment over any other, but to emphasize its value and to advocate its

adoption in combination with other recognized methods; after all, it is not so much a method of treatment in itself, as an aid to existing methods. In my opinion the ready amelioration of the pain and discomfort of a profuse acute urethritis, and its rather remarkable effect in clearing up an intractable case when other methods have failed, in themselves warrant an extension of its use and encourage further trial.

Gonorrhœa and its cure, rapid, medium, or delayed, is not a subject to eulogize about, and what will benefit one lot of cases has been found quite useless in another, but I believe that these intravenous injections of acriflavine are of considerable value, and that the results are distinctly encouraging. One point in their favour is that they afford a specially valuable alternative treatment in those cases when, owing to hypospadias or urethral abnormality, irrigations are rendered difficult or impossible, and the same holds good when these are contra-indicated by the development of an acute epididymitis or other complication.

The technique used is simple in the extreme and within the scope of any practitioner. The apparatus consists of the old "606" stand with two glass containers, graduated, of 300 cubic centimetres capacity with a single runner-tube from each, with a glass window about a foot from the end, into which is fitted a Harrison tap. The needles used were ordinary Weinstraud pattern for intravenous work with short point and slightly convex edges. Care must be taken to see that the needles fit the Harrison tap accurately, and that no leakage occurs under pressure. The tops of the glass containers are covered with a four-layer thickness of ordinary sterile gauze, which is fastened round the outside with tape, or, better still, a rubber ring. The Harrison tap may be replaced at will with a ratchet clip and needle adapter, and this arrangement works extremely well.

The solution used is a freshly prepared sterile solution of acriflavine, 1 in 1,000 in normal saline. The solution is poured into the containers through the gauze, which acts as a filter and shuts off possible contamination. Care is taken to eliminate all air bubbles from the rubber tubing.

When it is certain that all air has been eliminated, the flow is stopped by means of the Harrison tap or ratchet clip, as the case may be. The needle, which is kept in a bowl of spirit, is then fitted on and the fluid run through it.

A convenient vein on the front of the arm is then selected, and the needle inserted, and the desired amount of solution is allowed to run in—the usual speed being about fifty cubic centimetres per minute. In cold climate the solution must be warmed to above blood heat, at which temperature the solution should be injected.

An Arkansas stone, on which the needles can be kept sharp, is an absolute necessity, as, though there is little difficulty in puncturing the vein with a sharp needle, a blunt weapon does not pierce the skin easily, and pushes the vein in front of it. The characteristic dimple, which vanishes as the needle is inserted, is the best indication of successful puncture.

It is important that the first injection should be given as early as possible when the case is seen.

Two hundred and fifty cubic centimetres were usually given on the first occasion, and after that 300 cubic centimetres at intervals of about five days—more than three or four injections were rarely necessary—there were, as a rule, no marked reactions and no ill-effects following the injection; one case had a

high temperature on the evening of the injection, and next day still had a pronounced tint of yellow in his face and some vomiting; he was kept in bed on low diet, and eliminatory treatment was prescribed, and he rapidly shook off the effects of what may have been an overdose—in this case it was observed that acriflavine was still being excreted in the urine eight days after the injection, the usual period being four to five days.

N.B.—In this connexion it should be observed that the face often assumes a slight yellowish colour towards the end of the injection, but this soon passes off, and is apparently due to the presence of the drug in the capillary circulation.

This treatment was used in seventy-nine cases altogether with a total of 188 injections, but many of these could not be traced to their conclusion owing to the evacuation, and my own transfer prevented me from following up others.

The following were among the cases treated:—

Name	Number of injections	Date of admission	Date of first injection	Date of discharge from hospital	Number of days in hospital	Remarks
Pte. B. ..	3	11.10.21	3.1.22	24.1.22	21 days after commencement of injection	Chronic case
Pte. L. ..	4	2.1.22	3.1.22	3.2.22	32	Acute case
Pte. L. ..	2	14.11.21	5.1.22	Cured of gonorrhœa, 28.1.22., V.S. supervened	23 days after commencement of injection	Chronic case
Pte. A. ..	4	2.1.22	5.1.22	28.2.22	38	Seven previous admissions for gonorrhœa
Pte. M. ..	4	5.1.22	5.1.22	21.2.22	48	Acute case
Cpl. M. ..	2	7.1.22	7.1.22	9.2.22	34	Acute case
Pte. D. ..	3	5.1.22	9.1.22	28.2.22	55	Acute case, relapsed
Sig. O. ..	1	16.11.21	10.1.22	27.1.22	14 days after commencement of injection	..
Cpl. B. ..	4	9.1.22	12.1.22	24.2.22	47	Acute case
Pte. C. ..	1	23.11.21	12.1.22	21.1.22	10 days after commencement of injection	..
Pte. H. ..	4	12.1.22	12.1.22	7.2.22	27	Acute case
Pte. T. ..	3	4.1.22	12.1.22	15.2.22	35	Acute case
Pte. M. ..	3	19.1.22	21.1.22	14.2.22	27	Acute case
L.-Cpl. J...	2	18.1.22	21.1.22	7.2.22	21	Acute case
Boy D. ..	2	19.1.22	21.1.22	10.2.22	23	Acute case
Pte. C. ..	4	21.1.22	21.1.22	25.2.22	36	Acute case with epididymitis
Pte. M. ..	2	29.1.22	30.1.22	14.2.22	17	Relapse
S. N. L. ..	2	29.1.22	30.1.22	14.2.22	17	Relapse
A. C. I. P.	2	30.1.22	30.1.22	14.2.22	16	Relapse
Pte. G. ..	2	11.12.21	3.2.22	14.2.22	11 days after commencement of injection	Chronic case
Pte. C. ..	3	3.2.22	6.2.22	23.2.22	21 days	Relapse complicated by epididymitis
Pte. A. ..	2	1.2.22	11.2.22	28.2.22	20 days	Relapse
Pte. R. ..	2	27.1.22	6.2 22	24.2.22	29 days	Relapse complicated by epididymitis
L.-Cpl. G...	2	10.2.22	11.2.22	21.2.22	12 days	Relapse

N.B.—These are twenty-four cases (which were discharged from hospital apparently cured after the usual tests) which show the possibilities of the treatment. The twelve acute cases admitted, and placed under treatment within a few days of infection, show an average period in hospital of thirty-four days. The chronic cases had been lingering on under ordinary treatment. The relapses had been under ordinary treatment elsewhere.

THREE CASES OF TROPICAL SORE.

BY MAJOR L. A. ANDREWS.
Royal Army Medical Corps.

I THINK the following cases are of interest because they illustrate how effective a simple method of treatment may occasionally prove to be for a disease whose resistance to cure may be judged not only by the number of remedies recommended, but also by the elaborate technique sometimes considered necessary.

The line of treatment was that recommended by Byam and Archibald and others, but no details are given in any textbook I have seen.

Case 1. Pte. E. B. C.—Admitted to Military Hospital, York, on May 11, 1922.

History.—Served overseas in Basra and Baghdad from March 8, 1921, to January 26, 1922, when he embarked for the United Kingdom. Suffered from dysentery in November, 1921, and was treated with injections of emetine. In December, 1921, noticed a "red mark" on the right side of his chin; the mark gradually increased in size and eventually formed a "lump."

He reported "sick" several times, and was usually treated with local application of iodine, with no beneficial results. The "lump" gradually increased in size, but did not ulcerate. He arrived in England on March 16, 1922, and was recommended by his father to treat the "lump" with oleate of mercury. This he did for two or three weeks without deriving any benefit. Subsequently he cut himself whilst shaving, and a purulent discharge from the sore followed shortly afterwards. Since arrival in the United Kingdom he has been in hospital as a query dysentery, and was posted to York on May 3, 1922.

Condition on Admission.—Red inflamed area, about the size of a half-crown on the right side of the chin, a definite nodule could be felt with considerable induration; there was a scab in the centre of this area, and desquamating skin over the remainder; considerable enlargement of the submental gland and slight generalized adenitis.

On microscopic examination Leishman-Donovan bodies were detected.

Treatment.—Hot fomentations applied to sore to endeavour to clean the ulcer. On May 26, 1922, an ointment containing antimony tartrate two per cent made up with benzoated lard, spread on lint the exact size of the sore was applied morning and evening. The ointment caused considerable pain on the second application, and the following day the appearance of the sore was not encouraging. There were numerous bullæ, some purulent, surrounded by a area of acute inflammation.

No more antimony was used, but the ulcer treated as an ordinary granulating wound. Healing was uninterrupted, and the patient was discharged to duty on

June 7, 1922, with only a slight scarring persisting, thirteen days after the application of the antimony ointment.

Case 2. Pte. W. C.—Admitted to the Surgical Division of the Military Hospital, York, for malunited fracture right wrist on July 28, 1922, and tranferred to my wards on July 30, 1922.

History.—Served in Baghdad from October, 1920, until May, 1922. In April, 1922, noticed a small sore on the right forearm, but did not report sick, and received no treatment. The sore did not heal, and he was admitted to this hospital for surgical treatment of a malunited fracture.

Condition on Admission.—Typical granulating oriental sore on inner surface of right forearm, microscopic examination revealed numerous Leishman-Donovan bodies.

Treatment.—As the reaction was so severe in the first case, antimony ointment was only applied once daily; no severe reaction took place, and the sore did not make much progress. Subsequently the antimony ointment was applied twice daily until a severe reaction occurred; the sore then healed, leaving practically no scar. Patient was retransferred to the Surgical Division on September 30, 1922.

Case 3. Serjt. A. E. C.—Was transferred to York Military Hospital from a Military Hospital on October 24, 1922,

History.—Served in Baghdad from October, 1920, until March, 1922. In January, 1922, he noticed several small "pimples" on the back of his right hand and on his right elbow. After a week or so he reported "sick," and was given zinc ointment to apply, but he derived no benefit. On March 18 he went to Basra, and embarked for the United Kingdom on March 30, 1922. He arrived in England on April 28, 1922, and after a month's furlough reported for duty. The condition had been getting progressively worse, so he reported "sick" on arrival at his new station.

Various ointments were employed, and for several weeks he was excused duty and eventually admitted to a Military Hospital on September 27, 1922.

There he received various treatments, including paraffin collosol iodine but with no definite improvement, and he was transferred to this hospital on October 24, 1922.

Condition on Admission.—Typical oriental sore on the right elbow, two inches long and one inch wide, granulating and discharging pus, and a second sore over the dorsum of the carpo-phalangeal joint of the index finger of the right hand.

The exudate from both sores showed numerous Leishman-Donovan bodies.

Treatment.—When the sores had been cleaned up by means of fomentations, profiting by my experience in the first two cases, antimony ointment was pushed vigorously; after the third day a very severe local reaction occurred, very painful with severe blistering. Antimony was then discontinued, and the sores treated with lotio rubra; both sores had completely healed with a healthy scar within a fortnight.

The technique of this treatment presents no difficulties, but the patient should be warned beforehand that a severe reaction will occur, as the first case caused both the patient and myself considerable apprehension.

I am much indebted to Major A. N. R. McNeill, D.S.O., for examining the specimens and demonstrating the Leishman-Donovan bodies in each case, and to Lieutenant-Colonel R. L. Popham, R.A.M.C., commanding the Military Hospital, for permission to publish these cases.

THE BRITISH MANUFACTURE OF INSULIN.

THE following communication has been received from Messrs. Burroughs Wellcome and Co. :—

Insulin is a pancreatic active principle discovered by workers in the University of Toronto and used in the treatment of *diabetes mellitus.* The British patent rights are vested in the Medical Research Council, who have issued licences for its manufacture on condition that each batch must, before issue, be biologically tested under the supervision of the Council. It is under this licence and condition that "Wellcome" Brand Insulin is manufactured and issued by Burroughs Wellcome and Co.

"Wellcome" Brand Insulin is a sterile, watery solution of a material obtained from fresh pancreas by a complicated fractionation. It is issued in rubber-capped phials containing 100 units in five cubic centimetres of solution. The dose is indicated in units and not volume of solution, the average dose being ten units injected subcutaneously twice daily. The present price per dose of ten units is 2s. 6d.

Each unit injected is calculated to enable the patient to take at the ensuing meal one to four grammes of additional carbohydrate or its equivalent without excreting sugar.

With each phial of "Wellcome" Brand Insulin, Burroughs Wellcome and Co. issue a pamphlet, prepared under the direction of the Medical Research Council, and containing authoritative particulars as to the mode of administration, type of case for which Insulin is suitable, dosage, etc. Copies of this pamphlet will be sent to any medical practitioner who applies to Burroughs Wellcome and Co.

Owing to the present scarcity of supplies, the Medical Research Council point out that treatment should, for a time at least, be confined to severe cases of *diabetes mellitus.*

A special warning is issued that there is as yet no clear evidence of a *curative* action. Insulin enables the patient to metabolize a sufficient quantity of carbohydrate, but there is no ground for assuming that it can bring about a resumption of the normal function.

For this reason its use is not to be recommended, except as an emergency matter, without a clear prospect of continuing the administration for an indefinitely long period.

An emphatic warning is issued that Insulin is of service only in true *diabetes mellitus* and is definitely dangerous in the so-called renal glycosuria or *diabetes innocens.* The British Research Council state that administration by the alimentary canal has no demonstrable effect and that preparations of the pancreas recommended for administration by the mouth, whatever their effects may be, have not the action of Insulin.

Echoes of the Past.

A PENSIONER'S EARLY SERVICE.

By Colonel S. F. CLARK.

(*Continued from p.* 308.)

Towards the end of September, 1891, I was directed to hold myself in readiness for service in Aden, and on November 15 left Mhow for Bombay, where, three days later, I embarked on H.M.S. "Serapis." This was the ship in which King Edward, as Prince of Wales, had visited India. On this occasion the old trooper took a battalion to Aden in relief of the one there which was due for England.

Early on the 24th the "Serapis" anchored in Aden harbour, and those for the shore were landed in lighters. In my whole career I never saw baggage so badly treated as it was in this disembarkation, for everything was dropped from the ship's side twelve to fifteen feet into a lighter. An officer's uniform case might get a ponderous chest of armourer's tools flung on to it.

There were three positions for troops at Aden: Steamer Point, the Crater, and the Isthmus. The first named was much sought after, and was occupied mainly by the Garrison Artillery, who manned the forts. It was open to the sea breezes, overlooked the harbour, and had a golf course of loose sand with putting greens of cement.

I was ordered to the Crater for duty, and was kindly taken in by the bank manager there, with whom the medical officer who was being relieved was living. With the exception of the senior medical officer, for whom a bungalow at Steamer Point was allocated, medical officers had to find accommodation as best they could, and were practically dependent upon the hospitality of civilians or of officers of other arms of the Service for shelter.

At the end of one week, however, I was ordered to the Isthmus with a party of the King's (Liverpool) Regiment, which was going through its musketry training. The only good thing about this position was the officers' quarters, a large building high up, *outside* the shut-in, desolate spot which was the Isthmus position proper. There was a small hospital here, barracks for single men, and a series of married quarters, occupied by unfortunate non-commissioned officers of Indian Departmental Corps and their families. The surroundings of this place were enough to depress anybody, and not long afterwards it was abandoned owing to its unhealthiness.

After a stay of three weeks here I was relieved, and returned to the Crater and the banker's house. The headquarters of the infantry battalion was located at the Crater, and there was also a military hospital—or rather

a *station* hospital, as was the term used at that time—with two medical officers. The banker and I messed together, but in February, 1892, I was transferred to Steamer Point. Here I was kindly taken in by the C.R.E., who had one of the mat-shed sort of bungalows that were erected near the forts, the idea being to destroy them if necessary so as to give the guns a clear line of fire. We messed together, but dined at the Artillery Mess each weekly guest night.

The hospital at Steamer Point was officered by the senior medical officer and one junior. and was located up a hill; there was a site still higher on which it was proposed to erect a better hospital some day. In the hot weather the walk up was very thirst-provoking, and large patches of perspiration disfigured one's khaki uniform. Cloth uniform was worn only once a year, on the ceremonial parade which was held each New Year's Day in honour of the Empress of India; but the issue of tunics, cloth trousers, etc., went on just as if the troops were serving at a home station.

As rain very rarely fell at Aden, water was a valuable commodity, and was sold in the native shops. Each member of the garrison had a daily ration of distilled water for drinking and cooking, and of brackish water for washing, etc. The utility of the famous tanks seems problematical, for they were absolutely dry during the whole of my time at Aden, except for a few days after the one fall of rain that I saw there. On that occasion the whole population went to view these reservoirs, but all that they contained was a few inches of muddy water, with masses of leaves in all stages of decomposition floating in it. After this shower, however, a small plant, with little yellow blossoms, appeared all over the barren rocks of Aden. A little water will apparently make the desert anywhere bloom.

I performed all the usual duties of a junior medical officer, and for lack of some more qualified person coming forward, I superintended the practices of the church choir. For many months I conducted evening service each alternate Sunday, and by the attraction of dispensing with a sermon very nearly held my own with the chaplain in regard to the size of the congregation.

In August, 1892, I got a week's leave and visited Perim and ports on the Somali coast, while six months afterwards I had a most interesting trip in the same vessel—an Indian Government troopship—to Socotra, and various rarely-visited ports on the south coast of Arabia. The Sultan of Socotra, and the chiefs of the other areas, recognized a sort of British protectorate, and the Governor of Aden was visiting them in his official capacity. This voyage was a most pleasurable one, as few Europeans have landed on Socotra, or seen these Arabian communities.

In April, 1893, I obtained my first leave of over ten days since I had left England, and sailed for Sydney. Here I married, and was back again in Aden in three months—by myself. I was ordered to the Crater again, where the second chaplain on the establishment, who had arrived by now, was good enough to accommodate me, for the bank had failed and the friendly banker had gone.

The Crater was a cheerless place, and in September, 1893, I embarked for Bombay *en route* for the Sind district for duty.

Karachi was reached early in October, and as I was no longer a bachelor I had to look out for a house of my own, and by the kindness of the Officer Commanding 1st Royal Fusiliers I got a bungalow in the British Infantry Lines. This greatly annoyed the General Officer Commanding, who wanted this habitation for his daughter, who was about to be married. The rent was more than twenty-five per cent of my pay, but nevertheless I hired furniture, servants, and a horse and trap with driver, gave my share of dinner parties, and entered into everything that was going on, without borrowing money or leaving any bills unpaid.

Four days after my arrival in Karachi I left by rail for Hyderabad, Sind, in order to accompany a strong detachment of Fusiliers who marched down to rejoin headquarters at the seaport. On getting back to Karachi I got my house ready, and was joined by my wife at the end of the year.

Life in this garrison was very pleasant, as there was always plenty going on. I played golf and captained the gymkhana cricket team, while I did duty at the Station Hospital, was in medical charge of the 1st Royal Fusiliers, and also looked after the hospital for soldiers' wives and children, known as the Female Hospital.

In September, 1894, I received orders to embark for England, tour-expired, on January 5 next. On December 9 I left Karachi in an Indian Marine ship that was taking Lord Harris, the Governor, back to Bombay, a two-and-a-half days' trip. My wife had to come down by passenger boat, and the Principal Medical Officer told me that I might do as I liked till further notice. Bombay had no attractions, so after six days I was sent to Deolali at my own request. Here I put up in the quarters for married officers, did duty at the hospital, and inspected arrivals and departures. On January 3, 1895, my wife and I left for Bombay in separate trains, and embarked next day in the hired transport "Britannia," one of the P. and O. Company's Jubilee vessels, which sailed on the 5th, and arrived at Portsmouth on the 24th, without having called anywhere.

I never saw India again, and so missed the historic places which I had planned to visit during my next tour of service. In spite of my low rate of pay I had enjoyed life. Outside the large cities everything was very much cheaper than it seems to be now, and India was rightly termed the poor man's country.

That bugbear of the Service, orderly duty, was not oppressive, as the assistant surgeon (apothecary) on duty dealt with all ordinary matters until the medical officer's evening visit.

In those days every medical officer on return from foreign service had to report himself personally to the Director-General. I obtained one month's leave which was afterwards extended to March 31, on which day I should have reported myself at Dover, but as that day was a Sunday I took the extra day, and did not show myself until April 1. All would have

been well, but at that time an annual muster roll of every individual in the Army was taken on each March 31, and as my name did not appear on it the War Office asked questions. I was "told off" by the Director-General for imagining that Sunday did not count for reporting purposes, and was deprived of one day's pay and allowances.

To the great disappointment of my wife and myself, I was ordered to Canterbury, where I went into lodgings. I did hospital work and recruiting, and my recreations consisted of country walks and cricket, but in the middle of December I was transferred to Dover, where I went into rooms. By this time a medical officer's quarter had been established in the Shaft Barracks, which was allotted to me, but it was not suitable for a married man. I did exactly the same work as I had done six years before—duty at the hospital and medical charge of the Shaft Barracks. In January, 1896, I sat for my examination for promotion to major, and in May had one month's leave in London, during which I took the course of operative surgery that was also necessary. This had to be gone through at a recognized teacher's, who rushed it very considerably on this occasion, on the ground that a war scare was at hand.

In July I was ordered to Lydd for a month, where I took my wife and found comfortable accommodation in an old house. I had mobilization orders to join a bearer-company—a formation that has long been scrapped.

My wife and I had many friends in Dover, but the quarters in barracks made things so uncomfortable that I asked for a change, and was sent to Brighton at very short notice, as the medical officer there was on the sick list. I stayed in a hotel until the other medical officer had recovered, when I got three days' leave to return to Dover and bring my wife down to rooms that I had taken. This was in October, and next month the Director-General wanted to know why there were two medical officers at Brighton, instead of one, and directed a return to the normal establishment to be made. I was doing the usual duty in hospital and barracks, and for two months lived in daily expectation of getting an order to move, and eventually was transferred to Shorncliffe in January, 1897.

Two interesting things that occurred during my stay at Brighton were the destruction of the Chain Pier by a gale, and the procession of motor-cars from London, to celebrate their emancipation from the man-with-a-red-flag-in-front status. Of fifty-seven cars that started only three arrived at Brighton anywhere near each other. Some of the others limped in during the night, but a great many did not finish at all. Motors have improved a lot since then.

The frequent moves to which medical officers were liable was one of the curses of the Service to married officers, and I naturally went into rooms at Sandgate. To take a house would have been an imprudent proceeding. I had now completed ten years' service, and my pay rose to 15s. per diem, instead of the £250 which medical officers outside the Indian establishment received after five years in the Army.

At Shorncliffe I did the usual hospital and camp duties, which were relieved by a six weeks' holiday in the summer, followed by participation in military manœuvres in Sussex, in which all the troops wore their red or blue uniforms, with helmets, as khaki was still unknown in England. Quite a large force was assembled, and I was in medical charge of an infantry brigade.

On October 3 I was informed that I would be required for foreign service at Hong Kong, and next day I left for London to take over temporarily the duties of company officer and secretary to the Principal Medical Officer, Home District, in St. George's Barracks, Trafalgar Square, a building which no longer exists. I returned to my station in three weeks, and left it again on November 9 for Southampton via London. I thought it rather hard lines to be detailed as orderly officer for my last twenty-four hours. The duty ended at 9 a.m. on the 9th, but as my train left at 7 a.m. I cleared off at that hour, and embarked next day in the hired transport "Jelunga," of the British India Company's fleet.

It will be noted, perhaps, that after having been over six years in India, I did duty at home for only nineteen months, during which I had been in four different stations, with temporary transfer to two others.

It will be appropriate to mention now the so-called "strike of the Army doctors." Of course there was no refusal to work on the part of men on the active list, as that would have been gross insubordination, but in order to enforce reforms in the service upon a reluctant and unsympathetic War Office, the medical schools dissuaded young graduates from entering the Army. As the War Office found no way of filling the vacancies, it eventually granted one concession after another, but contentment in the Service was not attained until 1898, when Lord Lansdowne, Secretary of State for War, met the wishes of the medical officers in the most handsome manner, and initiated the great advance in every direction that has been made by the Army Medical Service since that year.

For years the medical officers had been in a very discontented state, and had many grievances—such as under-payment in India, undue amount of foreign service, constant moving about, general lack of proper status, and so on, and the climax came in 1886 when relative rank, which they held, was abolished in the Army by Royal Warrant. This brought the simmering discontent to the boil, as medical officers could get no satisfactory answer to the query as to what kind of rank they held in place of that which had been done away with.

When I was gazetted my name appeared in the *Army List* under the heading, "Medical Staff," and I was designated "surgeon, ranking with a captain." I found, however, that I was expected to describe myself on my visiting cards as "*Mr.* X, Medical Staff." Next came Surgeons Major,—not Surgeon-Majors,—those under twenty years' service being marked in parenthesis as ranking with Majors, and those over this period with Lieutenant-Colonels. The next grade was Brigade Surgeon, limited to

fifty in number, also ranking with Lieutenant-Colonels, while the Deputy Surgeons General and the Surgeons General ranked with Colonels and Major-Generals respectively. The Director-General had the same relative rank, or rather was described as also ranking with a Major-General. The Quartermasters had honorary rank as captains and lieutenants.

The first attempt to settle the controversy about rank was made in 1891, when compound titles were introduced, ranging from Surgeon Colonel to Surgeon Lieutenant, as the name Surgeon General was untouched. These designations gave little satisfaction, as they were meaningless to both soldier and civilian, who understood military titles and nothing else; so discontent still prevailed until 1898, when the officers, who were officially known as the *Army* Medical Staff, and the other ranks—the Medical Staff Corps—were welded together as the Royal Army Medical Corps, with the military titles which are now held.

The British Medical Association is entitled to the eternal gratitude of the Corps for the yeoman service which it rendered through all these years of controversy. For a long period the *British Medical Journal* had published letters from medical officers relating to the grievances of the services, but these ceased on its unification as a Royal Corps, and a spirit of contentment became evident, while reform after reform was introduced which started the Army Medical Service with giant strides on the road to increased efficiency which it is still traversing.

"Service dress" was introduced about 1902, with a weird system of distinguishing marks for the various commissioned ranks which no ordinary man could follow, and which was quickly scrapped for the method which is now in use. Up to this date marches and manœuvres were done in full dress—almost in "review order." About the same year military frock coats were introduced for all ranks—the purchase of this extra garment being the result of an effort to reduce officers' expenses.

As the "Jelunga" carried nothing but troops she went to Crete and then back to Malta, and so did not reach Hong Kong until Boxing Day. House accommodation was scarce, so for over a year I was a monthly boarder at the Hong Kong hotel, but then the General Officer Commanding troops, on being approached directly, gave permission for medical officers to live at the Peak. So far, only the Principal Medical Officer had been able to reside in this healthy locality, and he had blocked the efforts of his officers to do the same thing. The change had a marked effect on the health of the medical officers and their families, and for the rest of my tour I lived at the Peak, either in a furnished house or in a boarding establishment.

The military hospital was simply a portion of Wellington Barracks, while the hospital for women and children was merely part of the married quarters. There were no nursing sisters, but—unlike India—the "other ranks" of the Royal Army Medical Corps were present for duty. An old wooden line-of-battle ship, the "Meeanee," was moored in the harbour, and was used as the venereal wards of the hospital. This was my first work,

and afterwards I did duty on shore, and for some time was in charge of the hospital for women, and of officers and families. In April, 1898, the American fleet left Hong Kong to engage the Spaniards in Manila Bay, and as the flagship steamed past the "Meeanee," with her band playing the "Star-spangled Banner," and her crew full of enthusiasm, the British cheered them, and the Americans sprang into the rigging and replied with three of the heartiest cheers possible. This little send-off pleased the Yanks immensely.

In September of the same year I contracted acute bronchitis and malarial fever simultaneously and was admitted to the Government Civil Hospital, as the military establishment had no wards for officers. When I was well enough to travel I was granted two months' sick leave to Japan, but it was a long time before I was fully myself again. It may be of interest to state that it was my case that brought about the regulation empowering a medical board to grant sick leave from South China to Australia.

In 1899 I completed twelve years' service and was promoted to the rank of major, and in the same year I took part in a little warlike expedition in Kowloon and its hinterland, which had points of interest in it.

About this time the Far East bulked largely in the diplomatic circles of the world, and for a year or two a great fleet of warships, under different flags, was based on Hong Kong. Many formidable-looking Russian vessels passed through on their way to Port Arthur, where most of them were afterwards sunk by the Japanese.

In 1900 the Boxer rebellion broke out, and I went north with a reinforcement from the garrison for the expedition for the relief of the Pekin legations. The original Naval Relief Force had been badly mauled and pressed back by the Chinese, whose strength and equipment had been greatly underrated by the Allies, so that the whole column had a narrow escape from being annihilated. I was at first lent to the Navy to deal with the wounded of this Force, and on being relieved I received the thanks of the Senior Service. I returned to Hong Kong in three months on completion of duty, but suffering from the effects of having drunk bad water which was supposed to have been rendered harmless by distillation. Up to that time I had thought that this process made any water fit for drinking, but one is always learning.

Between plague, malaria, venereal disease, heat stroke, an epidemic of dengue, and other ills, there was always plenty of work at Hong Kong, but there was also plenty of play of all kinds.

I went to Japan on two months' leave in 1901, and also in 1902, and in January, 1903, I left for England tour-expired. I was sent home in a Blue Funnel ship, in charge of a lunatic (temporarily) officer and two men similarly afflicted, and had a small party as escort. This was quite an interesting voyage, as at Singapore and Penang the vessel filled up with pilgrims for Mecca, who were landed at Jeddah, a port with a most hair-raising assortment of coral reefs, some above water and some not.

After a week at Amsterdam the ship reached London, fifty-one days after leaving Hong Kong. I have spent much time at sea, but never have I seen the waves "mountains high" as they were in the Bay of Biscay on this voyage. One marvelled how any small sailing ship of our forefathers ever lived through such tremendous seas.

After two months' leave I was sent to Devonport where I was appointed medical officer in charge of effective troops, the military prison, and recruiting. My many changes of station on Home Service had made me chary of taking a house, so I went into rooms, a custom which I followed during the whole of my military career. It was only when I was abroad that I had the comforts of a house of my own.

From the middle of July (1903) I spent ten weeks on Salisbury Plain with the infantry of the Devonport Garrison, and in the following summer went to Dartmoor with the same troops for two weeks. On the way up I met with the kind of accident that has killed many a man—my horse bolted and threw me, rendering me unconscious, and necessitating my return to Devonport for three or four days.

During my time at Devonport I passed the examination for promotion to Lieutenant-Colonel, and also obtained the D.P.H., upon which I applied for an appointment as sanitary officer at home or in South Africa. These billets had been in existence since the close of the Boer War.

In July, 1904, I was warned for service in S. Africa, and left my station on leave about the end of November, and embarked at Southampton, on Christmas eve, in a Union Castle mail boat, paying for the passages of my wife and son. I was originally detailed to go in a troopship, but as it had no room for my family I was allowed to sail in the liner which was carrying a big overflow from the transport. In those days officers' wives and children had no claim for conveyance to or from foreign stations, a rule which hit many very hard.

Cape Town was reached on January 10, 1905, where I found that I was to be sanitary officer for Cape Colony, and was given a military bungalow at Wynberg to live in. Here I thought I was a fixture, but in the following October I was transferred to Bloemfontein as sanitary officer for Orange River Colony and Natal, and available for Cape Colony if required. I again got a military bungalow, but as it was haunted (a genuine case) I gladly shifted to the next one when it became vacant, and I perforce became a believer in visitations from the spirit world.

My duties entailed a good deal of railway travelling, and I attended camps of exercise, and for recreation played in a number of cricket matches.

In 1908 I went to Australia on six months' leave, and during my absence my appointment was abolished, owing to the reduction of the garrison in South Africa; so shortly after my return I was transferred to Middelburg, Cape Colony, where I soon afterwards became Officer Commanding military hospital. The troops gradually went away, and the small remnant that remained to the end had quite a good time. By September, 1909, the

station was closed, and I was on the move again with a formidable amount of animate and inanimate impedimenta. At the end of the month I arrived at Pietermaritzburg, and took over command of the military hospital. For the first time during the tour of service I did not live in a Government quarter, but took a house in the town.

Life in Natal was pleasant, but beyond being warned to be in readiness for service against Dinizulu, who was disposed of by the local forces without any Imperial help, nothing of moment occurred, and on January 9, 1911, I embarked at Durban, tour expired, on the hired transport "Rohilla," from which I disembarked at Southampton on February 2.

As this narrative has now been brought up to comparatively recent times, it will suffice to say that I was appointed Specialist Sanitary Officer in one of the Home Commands, and on my promotion to Lieutenant-Colonel at the end of 1911 I became Medical Inspector of Recruits in the same Command. I was employed with troops in the railway and miners' strikes of 1911 and 1912, and also "assisted" at the Coronation of King George, and at the Investiture of the Prince of Wales at Carnarvon. I was also at several Staff tours, and was meditating retirement from the Service in preference to going abroad again, when August, 1914, arrived. I went to France with the "Old Contemptibles," was promoted Colonel in the next year, and after various vicissitudes retired into private life on the conclusion of the Great War.

Current Literature.

The Reductase Test of Barthel and Jensen. By J. Smith. *Journal of Hygiene*, November 1922. The reducing power of milk for methylene blue was first demonstrated by Neisser and Wechsburg. This reaction, the direct reductase test, is supposed to be due to the bacteria and leucocytes present in the milk, and Barthel and Jensen made use of the reaction as a means of estimating the approximate number of bacteria per cubic centimetre of milk. Cunningham and Thorpe found the reductase test compared favourably with counts made by the dilution method and was a useful test when a rough estimate was required quickly. Smith made a number of experiments comparing the number of colonies growing on agar plates, incubated at 38° C. for forty-eight hours, inoculated with various dilutions of milk, with the reductase test as employed by Jensen. A solution of methylene blue was made by dissolving one tablet (Blanenfeldt and Tvede, Copenhagen) in 200 cubic centimetres of sterile water. The milk samples were well shaken and 40 cubic centimetres of each sample pipetted into tubes; one cubic centimetre of the methylene blue solution was added to each tube, the tubes were corked and well shaken and then

incubated in a water bath at 38° C. According to Jensen if the milk is not decolorized in five and a half hours there are less than 500,000 organisms per cubic centimetre; if decolorized in five and a half but not in two hours, the milk contains 500,000 to 4,000,000 per cubic centimetre; decolorized in two hours but not in twenty minutes, there are 4,000,000 to 20,000,000 per cubic centimetre; decolorized in twenty minutes there are more than 20,000,000 per cubic centimetre. Smith found that the reductase test gave usually a higher estimate than the plate method; but when there are more than 100,000 organisms per cubic centimetre the reductase test may be regarded as accurate as the plate method.

A Device to find an Upper Limit for the Protein Requirement of Man when the Caloric Requirements are known. By G. F. Göthlin. *Skand. Arch. F. Physiol.* 1922. The author concludes that the protein requirement of the average normal man living on a mixed diet is amply covered if he receives 8·8 per cent of his total intake in calories in the form of protein. A man leading a sedentary life with an intake of 2,600 calories per day would therefore not require more than fifty-three grammes of protein. A man doing muscular work would not require more than sixty-one grammes of protein. These figures appear to be justified by the literature mentioned, but they are little more than half of those given by Voit and by Rubner.

Buccal Infection with B. Tuberculosis. By S. R. Gloyne. *Tubercle*, 1922. The author made experiments to find out

(1) Whether virulent tubercle bacilli are commonly present in the mouths of patients suffering from pulmonary tuberculosis.

(2) If so whether these organisms can be transferred to table utensils.

(3) Whether the ordinary washing of these utensils destroys the bacilli.

The mouth-washes of twenty sputum-positive cases were injected into guinea-pigs and positive results were obtained in ten cases, in eight of which the patient had profuse expectorations. Six of the spoons used by the twenty patients also gave positive guinea-pig results; all the six patients had profuse expectoration. Other table utensils were not found to be infected. Similar investigations of tea-cups and ward crockery washed in plain water were made and the results were invariably negative. Hence plain water if used in sufficient quantity appears to be a satisfactory sterilizing agent.

Bacteriological Investigations on Influenza. By B. Gosio and others. *Ann. dig.* 1922. These papers give the result of investigations carried out under the direction of Gosio, in the bacteriological laboratories of Public Health in Rome. The hæmophilia of *B. influenzæ* is specific apparently depending on the presence in the culture media of substances contained in the blood of certain animals such as the pigeon and the dove. The blood of other animals, guinea-pig, dog, horse, rabbit and man more or less

prevents the development of typical strains of *B. influenzæ*. Under the best conditions typical strains appear to die out in from three days to about a week. Typical strains do not ferment arbutin, mannose, lactose, saccharose, and very little galactose and maltose. They cause an abundant and constant fermentation of glucose and lævulose. In guinea-pigs the lesions caused by *B. influenzæ* appear to be due to its toxicity and are proportionate to the dose used. If the dose is fatal the lesions are characterized by intense congestion of all the internal organs, particularly of the lungs, kidneys and glands.

In man the behaviour of the micro-organism is different, probably because of microbic associations without which it would not perhaps proliferate and become so highly toxic. In fact an experimental pneumonia can be obtained in rabbits by inoculating into their air-passages cultures of *B. influenzæ* in symbiosis with streptococcus or diplococcus, while no lesion of the kind is observed if a double dose of each of the three germs is inoculated by itself. In the organisms of experimental animals *B. influenzæ* shows an intense leucotropism. The consequent leucocytosis and phagocytosis may save the animals if the dose inoculated is small; but if it is a large one or if the strain used be particularly toxic the leucocytosis becomes a means of diffusion of the germs and their toxins. Also in patients affected with influenza a hyperleucocytosis is a symptom of extreme gravity and generally precedes a fatal collapse of the patient's power of resistance.

Vaccination of Monkeys against Pneumococcus Type I Pneumonia by Means of Intratracheal Injection of Pneumococcus Type I Vaccine. By R. I. Cecil and G. I. Steffen. Public Health Reports, U.S. November 2, 1922. The authors in a previous paper showed that monkeys could be completely immunized against pneumococcus Type I pneumonia by three subcutaneous injections of pneumococcus Type I vaccine. Their results were in harmony with those of Cecil and Vaughan who vaccinated U.S. soldiers with subcutaneous injections of pneumococcus vaccine and found that pneumonia incidence was much lowered in these vaccinated organizations. Cecil and Vaughan commented on the severe reactions which often followed subcutaneous vaccination and thought the method would have to be improved before active immunization would be practical in civil life.

Cecil and Steffen then experimented for three years with modified pneumococcus vaccines, but did not find any so efficient as the original saline suspension of killed pneumococci. The work of Besredka on the production of local immunity in the respiratory tract by intratracheal injections of vaccine then led them to try whether by injecting the vaccine directly into the trachea a satisfactory immunity against lobar pneumonia could not be obtained.

The experiments were made with monkeys and a vaccine was prepared

in the usual way from a highly virulent pneumococcus Type I, capable of killing a mouse in a dose of 0·00000001 cubic centimetre of a twenty-four-hour broth culture. The neck of the monkey was shaved and the skin painted with iodine. The vaccine was then injected directly into the trachea with a sterile hypodermic syringe. Immediately after the injection the operating board was raised in order to facilitate the descent of the vaccine into the lungs. Injections were given at intervals of five to seven days and the immunity of the monkeys was tested two or three weeks after the third administration of vaccine by inoculating them intratracheally with small doses (0·001 to 0·0001 cubic centimetre) of living virulent pneumococcus culture.

The authors conclude that the intratracheal inoculation of monkeys with three doses (total 12,000,000,000 pneumococci) of pneumococcus Type I vaccine renders them completely immune against experimental pneumococcus Type I pneumonia.

The mere spraying of the throat with pneumococcus vaccine will not produce complete immunity against pneumonia in monkeys. Failure in these circumstances may be due to the fact that the monkey, by closing off the naso-pharynx, prevents the vaporized vaccine from entering the trachea. This difficulty, of course, would not be encountered in vaccinating man by the spray method.

The immunity established by intratracheal injections of pneumococcus vaccine is probably cellular in character. Little or no protective substance against pneumococcus can be demonstrated in the serum of monkeys vaccinated by this method.

Microscopic Demonstration of Bacteria in the Lesions of Epidemic (lethargic) Encephalitis. By Edward C. Rosenow and George H. Jackson. *Journal of Infectious Diseases.* February 1923. A diplostreptococcus was isolated by Rosenow from the brain of a case of acute encephalitis which occurred during the epidemic of poliomyelitis in 1916. In 1918 a similar organism was demonstrated in the lesions of the brain and the cord of two children in the same family, who died with symptoms of acute encephalo-myelitis.

Rosenow and Jackson then examined sections of the brain and medulla from twenty-one cases of encephalitis which occurred in widely separated regions. The diagnosis of the cases was either lethargic or epidemic encephalitis. The lesions were typical of encephalitis in all and undoubted cocci or diplococci, sometimes in short chains, were found in each of the twenty-one cases. No other bacteria were found. The shape, size, and grouping of the organisms varied greatly. Large, medium-sized and small diplococci were seen. Some were too small to photograph and were not unlike the filtrable bodies isolated from cases of acute encephalitis by Loewe and Strauss and others. Rosenow and Jackson believe the organisms found by them to have a causal relationship.

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Non-Specific Desensitization. By C. H. Kellaway and S. J. Cowell. *Brit. Journ. Exper. Pathol.*, 1922, 3, 268. (1) The intravenous injection of normal guinea-pig serum into actively sensitive guinea-pigs causes a degree of protection against the specific antigen which is only of short duration. (2) This loss and subsequent return of the sensitiveness of the anaphylactic animal is explained by parallel changes in the sensitiveness of the plain muscle. (3) The changes in the sensitiveness of the plain muscle are probably due to physical changes occurring in the muscle cells. (4) The demonstrable antibody of the serum is greatly diminished in amount very soon after the injection of serum, and is not restored till long after the sensitiveness of the plain muscle has returned to its original value. (5) The injection of guinea-pig serum into guinea-pigs with a high titre of circulating antibody is followed by the appearance of enhanced sensitiveness of the whole animal during two periods—one shortly after the injection, and a second after the restoration of the sensitiveness of the plain muscle but before the return to normal of the demonstrable circulating antibody.

Application of the Absorption of Agglutinin Test to the Serological Study of Pneumococci. By R. R. Armstrong. *Brit. Journ. Exper. Pathol.*, 1922, 3, 287. (1) Agglutination by specific pneumococcal serums is not invariably sufficient for recognition of type. The absorption of agglutinin test affords satisfactory confirmation. (2) The use of the absorption test has led to the identification of sub-types of Type I and Type III pneumococcus comparable to the sub-types 2A and 2B of Type II already differentiated by Avery. (3) Sub-type strains exhibit limited specificity within the group to which they belong; in no case has a serum prepared against a sub-type pneumococcus agglutinated the standard type strain. (4) The serum of rabbits inoculated with the standard Type II pneumococcus is particularly rich in subordinate agglutinins. This property is shared by sub-type 2 strains in general, since the sub-type serums were found to agglutinate pneumococcus strains which had failed to react with any of the three standard serums. By this means a degree of serological relationship between the Type II group and salivary streptococci of the *Streptococcus viridans* group has been demonstrated. Salivary streptococci are sharply differentiated from pneumococci by their vigorous agglutinogenic properties.

On Vitamin Underfeeding. By W. Cramer. *Brit. Journ. Exper. Pathol.*, 1922, 3, 298. Observations have been made on a stock of rats which have been kept through many generations on a natural diet, the vitamin content of which, though restricted, is adequate to enable them to grow and breed and to prevent the occurrence of obvious ill-health. A comparison of rats from these stocks with animals from a stock fed on the same diet supplemented with an abundant supply of vitamins shows that there is such a condition as "vitamin underfeeding" and that it may occur

on a natural diet. Such vitamin underfeeding does not lead to any obvious ill-health : the animals are in appearance normal, healthy animals. But vitamin underfeeding, especially if it has occurred in infancy, impresses itself upon the organism as a lasting weakness which only manifests itself when the organism is exposed to a strain. Hence the importance of insuring an abundant supply of vitamins in the food, especially to the pregnant and lactating mother and to the growing child. The fact that vitamins have a positive, stimulating drug-like action and thus act as food hormones is put forward to replace the present conception of their mode of action. These conclusions open up a social aspect of the vitamin problem which has hitherto not been recognized, but which is at least as important as the actual production of diseases by a severe vitamin deficiency. They suggest that the physical make-up of a community is determined largely by the ease and regularity with which an abundant supply of vitamins is secured to the pregnant and nursing mother and to the growing child.

On the Antitryptic Action of the Blood. By E. Beaton. *Brit. Journ. Exper. Pathol.*, 1922, 5, 224. (1) The albumin-fraction of the serum-proteins is more antitryptic than the globulin fraction, and in human serum the inhibiting factor lies almost entirely in the former. (2) This association of the antitryptic action with the albumin-fraction applies also to the abnormal increment of antitrypsin occurring in certain conditions of disease. (3) There is no proportional relation between the antitryptic power of the serum and the concentration, either of the total protein or of the albumin; but, on the contrary, an increased inhibitory capacity may be accompanied by a diminished protein content, and especially by a diminished albumin content. (4) The antitryptic power of the serum cannot be conceived as due to a simple diversion of the enzyme by the protein as such, but must be dependent upon some at present unrecognized character of this protein.

The Serological Relationships of the Paracholera Vibrios to Vibrio Choleræ, and the Serological Races of the Paracholera Group. By T. J. Mackie. *Brit. Journ. Exper. Pathol.*, 1922, 5, 231. (1) The paracholera vibrios comprise a group which is not serologically homogeneous, but, in addition to *V. paracholeræ A* and *B* already described, represents a considerable number of serological races precisely differentiated by agglutination reactions. (2) By direct agglutination tests using plain saline emulsions and incubating at 37° C. for two hours the paracholera vibrios are distinctly differentiated from *V. choleræ*. (3) *V. choleræ* antiserum exhibits apparent coagglutination under certain conditions towards *V. paracholeræ A* and certain similar types; this effect develops more slowly than the agglutination of the homologous organism and is of lesser degree and of lower end-titre; it is most markedly elicited when formol-broth emulsions are used and the tubes are incubated first at 55° C.

The anti-bactericidal Properties of Colloidal Silica. By S. L. Cummins. *Brit. Journ. Exper. Pathol.*, 1922, 5, 237. (1) Colloidal silica has the property of interfering with the bactericidal power of blood in marked degree. (2) It inhibits the action of "complement" in a hæmolytic mixture. This anti-complementary property is probably the character to which it owes its anti-bactericidal power. (3) These properties, together with its want of any demonstrable germicidal power, point to colloidal silica as likely to be of great use in blood cultures in cases of bacteriæmia.

Silicosis and Miner's Phthisis. By W. E. Gye and E. H. Kettle. *Brit. Journ. Exper. Pathol.*, 1922, 5, 241. Insoluble silica and silica sol., injected subcutaneously, cause a similar and characteristic lesion consisting of a central area of necrosis surrounded by a zone of leucocytes; around this again is a second annular zone of necrosis limited by macrophages and granulation tissue. Tubercle bacilli proliferate rapidly in the necrotic areas, a small dose of bacilli becoming a large dose. In this way silica aids in establishing a local infection.

Reviews.

THE SURGERY OF THE WAR, VOLUME II. Edited by Major-General W. G. Macpherson, K.C.M.G., C.B., LL.D.; Major-General Sir A. A. Bowlby, K.C.B., K.C.M.G., K.C.V.O.; Major-General Sir Cuthbert Wallace, K.C.M.G., C.B.; and Colonel Sir Crisp English, K.C.M.G. Pp. 604, with 7 coloured plates and numerous illustrations. Printed and published by His Majesty's Stationery Office, 1922. Price £1 5s.

FIRST NOTICE.

The writer of the present review has endeavoured to collect all the surgical literature of the War which has appeared in medical periodicals, in pamphlet and in book form, and in official documents. The mass is already stupendous, even when material appearing in the English language alone is considered. And its reduction into an assimilable form must be a labour involving some years. In our opinion the time for publication is not yet, putting aside the fact that end-results have not yet been realized in many instances. And the conviction imposes itself that the volumes of the Surgery of the War at present under review are not the last word, the judicious summary which they should be, but merely a new series of pamphlets, admirably written, no doubt, and summing up the situation to the present date, with a very careful selection and use of material, but still in pamphlet form.

The surgery of the War beyond all question awaits the individual genius who can deal gradually with the whole mass, who, partly from

personal experience, partly by the magic of a sympathetic imagination, can pass under his purview the wounded man from the trenches to the casualty clearing station, to the base hospital, to the reconstructional centre, and finally to his absorption into civil life again. The form adopted in this volume is too much in the style and manner of a textbook. It is not History in any aspect of that much-abused science. Moreover, our individual genius must be a man of leisure, and the leisured surgeon is either very young or rather old.

There are many honoured names among the authors of Vol. II, but also many who are less well known, and whose judgment might fairly be considered to lack fullness of experience.

It is not that we would impugn the ability of industry of the various writers who have contributed to this volume. But the limits imposed upon their work by considerations of economy, of space, and even of time, have made it impossible either to supply all the detail necessary in a textbook or to treat the wider aspects of war surgery. History is not written in this manner. Committees may produce fair textbooks, but not history. It is easy to recognize and blame partiality and prejudice in a historian, but it is to be questioned whether the most one-sided account of the surgery of the Great War written with the pen of an Erichsen or a Jacobson would not have been preferable to this impeccable scientific record, a good deal of which is made up of statistical and manipulative detail which might well have appeared in appendices.

The reviewer, however, feels that this attitude of disappointment is somewhat ungrateful to those whose labours have provided the material for the present volume. They have spoken according to their brief, and spoken well.

(1) The present volume opens with about forty pages on gunshot wounds of the head by Messrs. Trotter and Wagstaffe. Writing at the beginning of the nineteenth century Guthrie points out that "injuries of the head affecting the brain are difficult of distinction, doubtful in their character, treacherous in the course, and for the most part fatal in their results." The present record shows that while some parts of this indictment of head wounds is true, while the primary mortality is still very high, the secondary mortality due to sepsis is still higher than it need be, the number of complete recoveries still unfortunately low, yet the principles of treatment are now more clearly understood, principles which bear the impress of Harvey Cushing's excellent work on the Western Front.

The remarks on the nature of gunshots of the head and their manner of infliction are perfectly sound, if not altogether new, and a very proper emphasis is laid on the necessity of treating wounds of the head like other gunshot wounds by early excision and closure, provided that suitable operating and nursing conditions exist near the fighting line and the patient has not to be moved for at least a fortnight after operation. We are in entire agreement also with the recommendations as to anæsthetics

suitable to head cases. There was a vogue for local anæsthesia at one time due to the powerful influence of one or two surgeons, somewhat regardless of the fact that so many of these cases were half conscious and bemused, and often restless and noisy.

Cushing thinks that convalescence is shortened and the liability to headache and epilepsy lessened by opening an intact dura and allowing pulped brain and blood to escape. In giving this advice he does not seem to have realized what war conditions usually are. He himself might open the dura without accident, but it is quite unwise to recommend the procedure for all surgeons, and without some special indication of increasing intracranial pressure.

Most English authorities, including the writers of this article, take this view. On the other hand no exception is taken by them to the introduction of a finger into a wound to search for foreign bodies, a practice which Cushing justifiably made the subject of a protest. The longitudinal sinus syndrome and its treatment are adequately dealt with.

The Tables I, II and III are clear enough in themselves, but their relation to each other is not made very apparent. From them, however, it may be gathered:

(i) What a very small proportion of head wounds recovered sufficiently to return to the fighting line even in a long campaign.

(ii) That even of those cases which survive to reach hospital nearly a third (266 out of 740) die, and

(iii) That of all recoveries one quarter (142 out of 546) suffer from persistent organic complications.

The most interesting pages of this section are the last ten, which deal with the later stages of head injuries. They include such subjects as the late removal of foreign bodies, the results of mechanical injury to the brain, unresolved cerebral contusion, the symptoms and closure of cranial openings, and traumatic cerebral neuroses. It is upon questions of this kind that attention is still being concentrated, as Bathe Rawlings' recent papers show, and as to which finality has not been reached. To take another instance, the ultimate results of leaving a foreign body in the brain cannot be ascertained till after the lapse of some years from the infliction of the wound.

The five illustrations in this article are reproductions and need no comment.

(2) An article on Injuries of the Face and Jaws by Messrs. Gillies and Mendleson occupies the next sixty-four pages. The matter of this article is very much more commendable than the manner. Some of it reads like a clinical note book, and the writers have not been at the pains to translate such craft phrases as "occlusal relationship," "rubber dam slings," "a flap of mucosa from the homolateral upper labiogingival sulcus," and it is possible to come across such sentences as "hæmostasis at the time of operation is somewhat liable to be responsible for a post-operative

hæmatoma." But Mr. Gillies' work at the Queen's Hospital for facial injuries at Sidcup is well known, and a study of the pastels, plaster casts, dental prosthetic apparatus and photographic series at Sidcup merely increases instead of explaining the wonder of some of his results.

The article begins with a description of the nature and site of the various types of facial injury, followed by some paragraphs on methods of examination, including skiagraphy. The use of "standard" positions in the latter is very properly insisted upon, but the descriptions of these positions on p. 45 is frankly incomprehensible. In the second sentence, the head is supposed to be resting on its side on a horizontal plane, a sagittal plane of the head in fact. At right angles to this are the coronal planes of the head. How then can any of them be "below the mandible"? The only planes "below the mandible" are those parallel to the "norma basalis." And how can a ray "inclined at an angle of 60° in the coronal plane" be at right angles to the sagittal plane? And in the last sentence of the paragraph the ray makes angles of 85° and 70° not "in" but "with" the planes as referred to above. These are confusing statements.

There follows an excellent little summary of the anæsthetics of choice in face operations. It is shown how flaps and detached tissues may be injured by local anæsthesia, and how the special liability to septic broncho-pneumonia after, and suffocation during, an operation make intratracheal ether the best anæsthetic for this work.

The principles of early treatment are laid down in the next paragraphs. The methods of primary excision and suture are recommended, but primary suture must be made applicable to the local circumstances. In case of large loss no attempt should be made to bring parts together, but it is essential to cover raw areas, e.g., to sow mucous membrane to skin, to cover the stumps of the mandible with mucous membrane, and so on. Contraction of scars, which is such a hindrance to prosthesis later, is by these means avoided or minimized. Similarly early replacement of the fractured mandible is advised, so that proper occlusion of the teeth is maintained from the first. The writers are very much in favour of bone-grafting of the mandible. With the maxilla, on the other hand, the aim is to obtain firm union as soon as possible, so as to have a fixed base for mastication. Maxillary and mandibular fractures singly or combined are then treated of more in detail, and the importance of oral asepsis and suitable dieting is insisted upon. The watery loss by hypersecretion and outflow of saliva, characteristic of these injuries, must be met by a diet largely fluid in nature. The late treatment of gunshots of mandible and maxilla is largely a question of dental prosthesis, and for an adequate description of this work, copious illustration seems to be necessary. A verbal description leaves an incomplete and incorrect impression.

The section on plastic repair indicates clearly the limitations as well as the possibilities of restoration of the face parts. Mr. Gillies shows that the ideal is restoration in kind, skin for skin, cartilage for cartilage, etc.,

but that some part of the restoration must usually be mechanical also. Some of the interesting points in this long section may be mentioned: The choice at first of ribs, and then of the tibia, of the uninjured portion of mandible itself, and finally of the crest of ilium as the source of bone grafts for the mandible; the epithelial inlay; the use of cartilage from the fifth to the ninth costal cartilages for repair of the nose; in replacements of cavities, a scaffolding and a lining are just as necessary as a covering; skin transference by means of tubed flaps, illustrated in the repair of burns. Illustrations are copious, and, on the whole, with the exception of those on pp. 51 and 52, are clear.

(3) In a short chapter on wounds of the neck, Mr. Douglas Harmer recalls the fact that bullets may pass through the neck without doing much damage, a fact also noted by Holt in the South African War. Shell fragments, on the other hand, cause lacerated wounds which heal badly, and often leave sensible traces of their passage, in the shape of laryngeal stenoses, paralysis of the vocal cords and of the cervical and brachial nerves. Mr. Harmer finds that the large vessels frequently escape injury, and that wounds of the œsophagus show a remarkable freedom from complications. Injury to the vessels frequently ends fatally on the field, and œsophageal wounds, in the experience of other observers, are very often followed by rapid and widespread cellulitis. It is noted that alteration of the voice is present in a large number of cases of laryngeal wounds as an after-result, possibly in some cases due to a neurosis. Mr. Harmer also mentions an experiment carried out in the First Army in France, by wearing necklets of Japanese silk to protect the neck against shrapnel splinters. They were reported as affording some protection.

(4) Gunshots of the spine and of the peripheral nerves are described by Sir William Thorburn in the next fifty pages. The author begins with a comparison of bone injuries in the typical fracture dislocation of civil life with the injuries usually found in warfare, and points out that damage to the cord may be due either to indriven bone or metal, or the concussion wave of a high velocity missile. He thinks that many of the cracks and fissures found may be due to traction on ligaments. These types of fracture are illustrated by photographs of specimens in the War Collections at the College of Surgeons, and the deduction from them is that in estimating the degree of damage to the cord, skiagraphy is not of so much use as elsewhere, though, as is shown later, careful interpretation will yield much valuable information.

Injuries of the theca may or may not give rise to spinal meningitis, and are more likely to do so when the missile itself opens the theca. Adhesions giving rise to the so-called cysts, the infrequency of meningeal hæmorrhage in quantity sufficient to cause pressure, and the tendency of foreign bodies to slip about in the theca are noted. Among the injuries sustained by the cord itself contusion by the mere explosion of high explosives is noted, probably for the first time in military surgery. In describing the symptoms

of cord injury reference is made to the study of reflex function below the lesion by Collier, Head and Riddoch, and the mass reflex described by the last named is shown to be usually associated with total transverse lesions, whereas a more purposive reflex indicates partial lesions.

The difficulty of defining the actual cord injury having been insisted upon, it is obvious that the question of operative treatment depends chiefly upon collateral consideration, such as the presence of sepsis of bone fragments or a missile, or of a definite clinical complex such as the Brown-Séquard paralysis. Late operations are usually undertaken for removal of pressure of various kinds. It is significant that in cases of complete recovery the lesion was usually in the cauda equina. The case for or against laminectomy is well summed up on p. 135, followed by a paragraph dealing with the attempts which have been made to suture the divided cord, or to insert a graft. In view of the recognized inability of the cord to reproduce damaged tissue, the author does not think either procedure can give relief. After a few sentences on the treatment of spinal pain and of continued spasm, the author remarks that recovery in incomplete cases may be hastened by the encouragement of voluntary effort by means of the self-impelled wheel-chair, the go-cart and the swimming bath, which re-educates the damaged upper motor neurons. Lastly, the bladder as holding the key to life or death for the patient is dealt with and the importance of urinary sepsis, the influence upon prognosis of loss of bladder control, and the methods of evacuation without using the catheter are touched upon. The advantages of suprapubic cystotomy are pointed out. The chapter ends with some remarks on root lesions.

(5) Injuries to peripheral nerves is the subject of the next chapter, also by Sir William Thorburn. The nature of the injury is first described, and then the conditions subsequently found at operation. South African experience showed how common an experience it was to find complete physiological discontinuity without marked anatomical change, and how difficult it often was to know if actual division of the nerve had taken place. But in this campaign one of the chief lessons learnt was the regularity in the distribution of the ultimate fibres of a nerve trunk, so that, assuming regeneration by down growth of axons each axon must take its proper place and grow into its proper distal channel for complete regeneration to occur. The bearing of this discovery on certain surgical methods of repair is obvious. Any " shunting " is disastrous, lateral anastomosis is impossible, and the insertion of nerve grafts, autogenous or heterogeneous, an act of lively faith. Nor can it be a matter for surprise that in discussing the results of secondary suture the opinion is expressed that a perfect neurological recovery is rarely, if ever, obtained. The author emphasizes the necessity of early use and re-education of paralysed parts, with avoidance of hospitalism. The period of waiting, both before and after operation, is often prolonged and a good deal of subjective disturbance often arises. The chapter closes with an interesting account of causalgia. Since the

description of this dire affection by Weir Mitchell in 1864, it has only appeared in a few cases of gunshot wounds. The reviewer remembers one case after the South African War which came in the end to posterior rhizotomy. Sir William Thorburn calls attention to its close similarity to trigeminal neuralgia and to the theory that the changes are in part due to injuries of the vasomotor nerves of the arteries supplying the affected nerves. Leriche's decapsulation operation is based on this observation.

Sir George Makins has made the subject of gunshot wounds of the blood-vessels a special study, and Chapters VI, VII and VIII present a complete survey of the subject. It was probably for the purposes of this article, and of Sir George's monograph on wounds of blood-vessels, that the forms for recording cases of this nature were sent round to hospitals. The topographical distribution of these wounds is given in a table at the beginning of Chapter VI. In the consideration of the nature of wounds to vessels it is pointed out that localized contusions and non-penetrating injuries are peculiar to missile wounds, and may be followed by thrombosis and obliteration. Also that this form of injury may be more extensive than appears on inspection, leading to weak points in the vessel wall which may subsequently give way, or to thrombosis which may lower the nutrition of the parts supplied. In describing actual wounds of vessels the author constantly has in mind the possibility of suture. For example, he shows how the ragged margins of a lateral wound tend to assume a more or less regular oval outline favourable to subsequent suture. It is remarkable that "mechanical control by pressure from an adjacent piece of metal is common." In the treatment of hæmorrhage both primary and secondary, while no great changes in ordinary surgical procedures were made, much advance was made in treatment by "measures calculated to increase the total volume of fluid in the blood-vessels," by the infusion of whole blood or Bayliss' gum arabic solution, or the administration of large amounts of fluid by mouth or rectum. An interesting section is devoted to the effects on the parts supplied by a wound of a vessel. Short of actual anæmic gangrene, the occurrence of which is favoured by such factors as previous loss of blood, cold, infection, associated nerve injury and the like, obliteration of a large arterial trunk may cause lowered nutrition of a limb, an effect which falls most heavily on the muscles, and may lead to changes identical with those in Volkmann's ischæmia.

Some form of aneurysm occurs in 54·4 per cent of injuries to great arterial trunks. The formation, both of aneurysm from hæmatoma and of the various forms of arteriovenous aneurysm, with their comparative seriousness, is well described.

In dealing with wounds of blood-vessels provisional control by means of the elastic tourniquet, or by provisional ligature of the artery or by clamping is discussed, and the important question of simultaneous ligature of artery and vein is given a couple of pages. The author expresses the opinion that "the immediate advantage gained was definitely established, but of the ultimate effect on the limbs, information is still wanting."

Much was hoped for from suture of wounded vessels, and a certain amount of work was done in this direction which Sir George Makins summarizes as follows :—

(i) It is the ideal method of cure.

(ii) It can only be applied in the absence of sepsis.

(iii) From the second to the tenth day the arterial wall is not in a state suitable to suture.

(iv) A further period of waiting up to three months will make the operation easier.

(v) Wounds involving up to one-third of the calibre may be sutured. Beyond that end-to-end union is preferable.

In the treatment of traumatic aneurysms there is nothing that is new. It is pointed out that spontaneous consolidation, rare in traumatic aneurysms, practically never occurs in arteriovenous aneurysms. In the latter, however, the pressure in the arterial system expends its effects upon the veins and causes sometimes serious venous obstruction.

The above are a few of the many points of interest which have been taken from a chapter full of valuable information, and well illustrated with plain and coloured plates.

Chapters VII and VIII apply the foregoing observations to individual vessels of the neck, trunk and limbs. To each group is appended a statistical table with information relative to complications, mortality, etc. The author pauses at intervals, and sums up the facts noted in a series of "observations" which are the really interesting parts of these chapters, which otherwise contain much necessarily dry clinical and statistical material.

Terminal results in the case of ligature of the main vessels of the limbs are shown to be bad. Few men were able to return even to light duty, and most were to some extent permanently crippled. In the case of the carotids, on the other hand, apart from the risk of cerebral disturbance, injury and ligature of the carotid arteries leave no serious sequelæ.

Taking wounds of neck and limb vessels alone, it is noticeable that only those in which the track is small survive to reach the casualty clearing station. Wounds of the "explosive" kind probably all die of hæmorrhage on the field, and so rapidly that except in the case of the smaller limb vessels practically no cases of ligature for primary bleeding are seen. Taking special vessels, the interest of wounds of the carotids centres in the cerebral complications which follow them ; the subclavian vessels are difficult to control, and, moreover, are specially liable to entry of air into the veins during operation ; the anatomical arrangement of the nerves surrounding the axillary and brachial causes the factor of nerve injury to be added to that due to loss of blood supply, and leads to the development of many useless limbs ; control of the common femoral does not necessarily ensure control of the branches of the profunda femoris, and the terminal results of femoral ligature cannot be regarded as satisfactory ; with wounds

of the popliteal artery the incidence of anæmic gangrene is "enormously high," but the reported cases of suture give promise of better things; and wounds of the tibial arteries call for prompt attention, as owing to their anatomical situation secondary hæmorrhage is exceedingly common. Throughout these two chapters the reader gains two impressions. First, that the author is convinced of the superiority of suture to all other methods of vessel treatment, and secondly that he desires to emphasize the War lesson of the importance of copious bleeding at any stage of treatment in its effects upon operative procedures and terminal results. Probably in the future we shall have it laid down that in any wound of a large vessel the first thing to be thought of is the replacement by transfusion of any blood lost.

PLAYS FOR CHILDREN. Vol. I. "Blue Beard, and Haroun el Rashid." Vol. II. "St. George and the Dragon," and the "Sleeping Beauty." Vol. III. "Goldilocks and the Three Bears;" "Torquil MacFerron," "Thomas Olifant" and "Tyranny." By S. Lyle Cummins. With coloured frontispieces and other illustrations by G. L. Stampa. (Methuen and Co.) Demy 16mo. 1s. 6d. each volume.

To every parent, the intellectual growth of a child is ever a matter of interest. The manner of educating the young has undergone great changes within this generation, but whether for the better or the worse is a matter of opinion. Certainly, of late years, the rigid discipline and austerity of the family life, which was so characteristic of the last century, has given place to a freedom and atmosphere of real comradeship between parents and children which would astonish our grandmothers. The general result of this change is that children of to-day are more natural, less afraid of expressing themselves and, as a rule, much more sympathetic than were their parents at the same age. They are, too, free of a great deal of the shyness and prejudice which was the result of the old-time home discipline. It is no longer a sin to be heard as well as seen.

Typical of the newer ideas is the appearance of the three small books which are the subject of this notice. They are plays for children, intended to be staged by the players themselves according to their own ideas and with such properties as are available or can be improvized at home. The motive underlying their preparation is good, but it is questionable whether the author has made the best choice of subjects or manner of treating them. Names and phraseology seem in many places to be too advanced for young children. True, it is intended that the longest and hardest part should be taken by a grown-up upon whose strength the younger actors can lean for support, and also the other parts are some long and some short to suit all ages, and phrased in rhyme so as to make memorizing easy, still we are afraid that for many young children the plays will be found too difficult, and for the older ones too childish and uninteresting. We hope

that we are wrong, and that they may find many enthusiastic casts to play them and many appreciative audiences to hear and see them. They certainly are worthy of trial, if only to foster confidence, good elocution and initiative among the younger folk. In places the author displays so good a sense of humour and happiness of phrasing that we hope he will turn his dramatic talents to writing a serious or even comic play for adults. In that direction, we think he will find a worthier field for his ability than in catering for the schoolroom, though we hardly expect him to allow amateurs to act such a play without payment of any fee, which he so kindly does in respect of these plays to be played by children. The Corps has produced some men of mark in the sphere of science, we see now no reason why it should not produce a playwright of some merit, certainly the environment of its activities is wanting neither of pathos, comedy nor tragedy. R. H. F.

THE DIAGNOSIS AND TREATMENT OF TROPICAL DISEASES. By. E. R. Stitt, M.D., Sc.D., etc. Fourth edition. Pp. xiii + 622, 159 illustrations. Price 18s. net. H. K. Lewis and Co.

The publication of still another edition of this popular pocket manual is a sufficient indication that it supplies a recognized need. The features of the earlier editions which ensured its popularity and success are retained with additions which increase its usefulness and scope. Six new chapters have been added dealing with epidemic jaundice; rat bite fever; tularæmia; trench fever; and diagnostics of tropical joint, muscle and bone lesions. The practical and helpful chapter on diagnostic problems and procedures has been amplified and enlarged.

The book is well up to date as regards the diagnosis and treatment of disease, and recent work and views are sufficiently presented. The effects of "Bayer 205" on animals infected with "human" trypanosomes is recorded, and the carbon tetrachloride treatment for ancylostomiasis receives due recognition.

In some instances the illustrations have not been altered to conform with revisions of the text. On p. 35 the author appears to accept J. D. Thompson's explanations of the presence of malaria merozoites, and a gamete, in the same blood corpuscle, but in Plate I a representation of this phenomenon is labelled "Parthenogenesis." On p. 321 the author gives the original form of the generic name of the "Old World" hookworm, and states that this is correctly rendered, *Ancylostoma*. Nevertheless in the plate of Nematode Ova on p. 585, a hookworm egg is entitled in bold type "*Agchylostoma*." (As is well known this original spelling of Dubini's had to be altered, being contrary to the rules for transliteration of Greek into Latin). Several other similar discrepancies between illustrations and text were noticed.

Some of the portions of the book touching on medical zoology do not maintain the general high standard of the rest of the volume. The author does not appear to be familiar with the work of Bacot, and Nuttall, on lice, and there is no indication of the widely held belief in the unity of species of the head and body louse of man.

The section dealing with the classification, etc., of mosquitoes requires editing. In the drawing of an *Anopheles* larva on p. 22 the reviewer cannot distinguish the external clypeal hairs, and this descriptive title in the illustration seems to indicate the mouth brushes. It is difficult to understand what classification of mosquitoes is recognized by the author. In this connexion he gives a number of anopheline "genera" based on scale characters, and whatever may be said in favour of regarding such gradations as indicating conveniently recognizable groups, there are few

nowadays who would uphold their status as valid genera. The author's description of *Nyssorynchus* is unintelligible to the reviewer. He thought that the first "abdominal" was probably a misprint for "thoracic," but the same definition is found in the preceding edition of this work, and also in the latest edition of the author's companion practical volume, and so presumably it represents his considered opinion. The anopheline scale variations given are not sufficiently comprehensive to admit such species as *A. maculipes*, and yet space is found for the fantastic "genus" *Aldrichia*, which is described as having its abdomen completely covered with scales, a character unknown in any anopheline. As a matter of fact, *Aldrichia* was founded on a single individual sent from India and which is still in the British Museum. As it exists, the specimen is a monster with the head and thorax of an *Anopheles*, to which, by some devilish cantrip sleight, the abdomen of a culicine has been gummed on! The perpetuation of this fabulous genus illustrates the virtual immortality acquired by any zoological name when once it has gained admission to medical textbooks. It may express a fantastic conception, or the name itself may be demonstrably incorrect. It may go down under a fierce assault, with twenty mortal murders on its crown, yet up it will start again and again with all the persistence of the blood-boltered Banquo, to sear the eyeballs of the astounded beholders.

When another edition of this popular book is called for it is hoped that the author will see his way to revise the section on mosquitoes and bring it more into line with modern views, and so increase still further the usefulness of this excellent pocket manual.

Correspondence.

ROYAL SCHOOL FOR DAUGHTERS OF OFFICERS OF THE ARMY.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—Excessive taxation and the high cost of living tend to reduce the number of subscriptions which we can afford; it is hoped, however, that in spite of this inevitable reduction many will be inclined to assist the Royal School for the Daughters of Officers of the Army which is in urgent need of funds.

The School was founded and established at Bath after the Crimean and Mutiny Campaigns, and in the half century of its existence has educated over 1,600 girls. It was enlarged after the South African Campaign, and during the Great War extra accommodation was acquired for forty girls, but further expansion has become imperative if the School is to fulfil the purpose for which it was founded.

The benefits of the School are urgently needed, not only by the daughters of officers killed, but also by those whose fathers have been compelled to retire on small pensions from various causes—unless entry can be placed within their parents' small means, many girls will be denied the opportunity of qualifying for professions which may be their only means of existence in the future.

Under present conditions the School accommodates 160 girls of whom fifty-three are admitted at the rate of £20 a year. The rate for the remainder is £90 a year, which compares favourably with the fees charged by other schools of the same standing. The means of giving their daughters a first-class education at this moderate cost is thus a real boon to widows or officers with small incomes.

To enable the School to extend its benefits, it has been decided to enlarge the present buildings by the erection of a War Memorial Wing which besides providing a hall and additional class rooms will enable the School to raise its numbers to nearly 200.

The cost of this scheme will be about £20,000. Past and present pupils and friends of the School have subscribed £13,000, and if a proportion of the balance required can be raised within the next few months, building operations will be started without delay.

The Committee hope that the scheme will meet with the support of the public, as well as that of the officers, both past and present. Much has been done for the sons of officers who fell or were disabled in the war, but not so much for their daughters. In view of the ever increasing part which women now take in public life, and the responsibilities devolving on them, girls now require all the educational facilities which under former conditions were reserved for their brothers.

Contributions should be forwarded to the Secretary, Royal School, 25, Haymarket, London. They should be marked "Memorial Fund." Contributors in India can, if preferred, remit their donations to Messrs. Grindlay and Co., of Bombay, Calcutta and Simla, who have kindly consented to receive the same.

Panton House,
25, *Haymarket,*
London, S.W. 1.
April 16, 1923.

I am, etc.,
C. C. Munro, *General.*
Vice-President, Royal School for Daughters of Officers of the Army.

Notices.

EDITORIAL NOTICES.

The Editor will be glad to receive original communications upon professional subjects, travel, and personal experiences, etc. He will also be glad to receive items of news and information regarding matters of interest to the Corps from the various garrisons, districts, and commands at home and abroad.

A free issue of twenty-five reprints will be made to contributors of Original Communications and of twenty-five excerpts of Lectures, Travels and Proceedings of the United Services Medical Society.

Any demand for *reprints, additional to the above*, or for excerpts must be forwarded at the time of submission of the article for publication.

Matter intended for the Corps News should reach the Editor not later than the 15th of each month for the following month's issue. Notices of Births, Marriages, and Deaths are inserted free of charge to subscribers. All these communications should be written upon one side of the paper only; they should by preference be type-written; but, if not, all proper names should be written in capital letters (or printed) to avoid mistakes, and be addressed: The Editor, "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS." War Office, Whitehall, S.W.1.

MANAGER'S NOTICES.

The JOURNAL OF THE ROYAL ARMY MEDICAL CORPS is published monthly, a volume commencing on 1st July and 1st January of each year.

The Annual Subscription for the Journal and Corps News Supplement is £1 (which includes postage), and should commence either on 1st July or 1st January; but if a subscriber wishes to commence at any other month he may do so by paying for the odd months between 1st July and 1st January at the rate of 1s. 8d. (one shilling and eightpence) per copy. (All subscriptions are payable in advance.)

Single copies can be obtained at the rate of 2s. per copy.

The Corps News Supplement is also issued separately from the Journal, and can be subscribed for at the rate of 4s. (four shillings) per anuum, including postage. (All subscriptions are payable in advance.)

Subscriptions for the Corps News Supplement separate from the Journal cannot be accepted from Officers on the Active List unless they are also subscribing to the Journal.

Single copies can be obtained at the rate of 6d. per copy.

Cheques or Postal Orders for Subscriptions, etc., should be made payable to the "Hon. Manager, Journal R.A.M.C." and crossed "Holt & Co."

All communications regarding subscriptions, etc., should be addressed to THE HON. MANAGER, "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS," WAR OFFICE, WHITEHALL, S.W. 1.

CASE FOR BINDING VOLUMES.—Strong and useful cases for binding can be obtained from the publishers at the following rates: Covers, 3s. 9d. net; binding 3s. 9d.; postage extra.

In forwarding parts for binding the name and address of sender should be enclosed in parcel.

VOLUME XL. JUNE, 1923. No. 6.

Journal of the Royal Army Medical Corps.

Original Communications.

PURIFICATION OF DRINKING WATER IN THE FIELD.[1]

BY LIEUTENANT-COLONEL J. A. ANDERSON.
Royal Army Medical Corps.

AT the inception of this Group of the Society, questions of the adaptability of military sanitary measures to civil conditions and the possibility that many methods used with success in the Services could feasibly and advantageously be adopted in civil practice, promised a wide and interesting field of discussion. It is, therefore, from that point of view that I wish to consider my subject this evening, dealing with apparatus and methods rather than with military organization and administrative arrangements.

We have evolved in the Army, and especially during the recent war, methods of water purification at once simple and effective, and it seems to me that there must be problems of water supply in civil public health practice—perhaps not in municipalities nor in large communities, but almost certainly in small communities or under rural conditions—where simple purification methods of proved efficiency would be useful and acceptable.

The supreme importance to health of an adequate supply of pure water requires no argument. Under active service conditions this importance is even greater, and at the same time attainment is beset by many inherent difficulties. Choice of source may be restricted by military necessity or by climatic or geographical conditions; laboratory examination of a proposed source is usually impracticable; apparatus is limited by the need for mobi-

[1] Paper read before the Naval, Military and Air Force Hygiene Group of the Society of Medical Officers of Health.

lity; the time available for purification is often very short; and the number and range of impurities requiring removal may be considerably expanded.

For some time there used to exist two opposed schools of opinion; one looked upon all field service sources of water as being dangerous without purification, the other regarded water as dangerous only after recent specific contamination. The latter school has fortunately disappeared or been eradicated, and it is now an axiom of water supply in the field that *all* water must be regarded as dangerous until effectively purified. An additional advantage in accepting this principle as a basis for war policy lies in the fact that, if all water is to be purified, choice of source becomes much less limited. In other words, if one is prepared to purify *all* water, one is prepared to purify *any* water. Thus in France large supplies of potable water were obtained from heavily polluted canals or from local and obviously dangerous sources. Nevertheless, patently dangerous as were these sources of supply, they were probably very little—if at all—worse than numberless sources in everyday use in rural England.

Having accepted a policy of universal purification, laboratory examination of a proposed source becomes of negligible value—a consequence in no way to be deplored. Laboratory examinations still remain of essential value in controlling adequacy of chlorination, in determining the presence or absence of inorganic poisons, and in deciding upon the most effective purification methods for waters presenting any problems out of the ordinary.

The measures of purification usually required are clarification and sterilization. By clarification is meant the removal of suspended matter such as fine grit, sand, clay, organic debris, etc., substances whose harmful action upon the body is mechanical, and which can be removed by sedimentation or fairly coarse filtration. By sterilization is meant, not the complete removal of bacteria, but merely the production of a water showing no lactose-fermenting organisms in 100 cubic centimetres, and therefore presumably free from danger of conveying intestinal disease. In addition, special purification measures may be necessary in connexion with saline waters, the presence of inorganic poisons, or the possibility of parasitic infections.

The experience and necessities of the recent war led to the evolution of our present standard method of clarification and sterilization, a method which may be briefly described as "clarification by alum, sterilization by chlorine."

Before discussing this method and the variety of its application, it may be of interest if I refer for a moment to other methods of purification which have been suggested for use in the field or which have actually been in use, though now discarded. At this point I ought, perhaps, to make clear that my criticism of these methods is based upon their suitability for use in forward areas or with mobile troops; water purification at the base or on

the line of communication is merely the same problem as confronts any large community under peace conditions.

Removal of the grosser suspended impurities by means of alum has been a standard method of water purification in armies for many years and still forms the initial procedure in our present-day practice.

Removal of bacteria by filtration, using filter candles of unglazed porcelain or other material, as in the well-known types (Pasteur-Chamberland, Berkefeld, Doulton, etc.), was the standard method for water sterilization in the Army on the outbreak of war in 1914, and water-carts fitted with these filters formed practically the sole apparatus available on mobilization. A very short experience of these carts showed their utter unsuitability for use under active service conditions. The rapid lessening in output if the water was not most efficiently clarified, the necessity for periodical (twice-a-week) boiling of the candles, their fragility, and the frequent existence of cracks or pinholes (accidental or intentional) in the substance of the candle, all conspired to inefficiency, and the method was soon discarded—with few regrets.

Sterilization by heat is, of course, simply to boil the water. A slow procedure, wasteful of fuel, resulting in a distinctly unpalatable water, the method is recognized only as an emergency measure, the consumption of water in the form of tea being recommended when other means of sterilization are not available. In some form or other of heat-exchange apparatus, water sterilization by heat has been the subject of much experimental work. The principle of the apparatus is the separation of the incoming cold water from the outgoing hot water by a thin metal diaphragm, resulting in a cooling of the sterilized hot water to a drinkable temperature and a corresponding rise in the temperature of the cold water with a resulting economy in fuel consumption. The outfit, however, was cumbrous and the yield was small in proportion to size and weight. The method was adopted for some time in the United States Army and has recently been used in the U.S.A. for sterilizing drinking water on board the river and lake steamers, steam being there available for heating. The method has never been taken into use (except experimentally) in our Army.

The use of light, in the form of ultra-violet rays, is a recognized method of water sterilization, and municipal systems have been installed in certain towns in France. The portable plant shown in fig. 1 was used for a short time in the French Army.

The water is unaffected in any way, which is a definite advantage, but the apparatus is delicate, the yield is small, and some means of generating a fairly powerful electric current is necessary. At present the method is not a practicable one for use under field service conditions.

Chemical substances have been used at different times and in large variety for the sterilization of water in the field. In the majority of cases these methods have been introduced to solve the problem of individual supply, but a few have been used for bulk purification. Potassium per-

manganate was used as long ago as the Franco-German war of 1870 at that time against organic matter, not organisms), and has been widely used in India for "pinking" wells and tanks. Its action is slow and unreliable. Ozone is used in several municipal waterworks on the Continent, but, so far as I am aware, has never been adapted to army requirements. I think that it merits future consideration and experiment. The halogens are amongst the most powerful of the disinfectants, but, with the exception of chlorine, are better suited for individual methods than for bulk sterilization. Chlorine, however, is eminently suitable for the latter purpose, and consideration of its action and uses brings me to the methods employed in the Army at the present day.

The present standard method of water purification as practised in our Army on field service has already been summarized as "clarification by alum, sterilization by chlorine." In order to understand the various

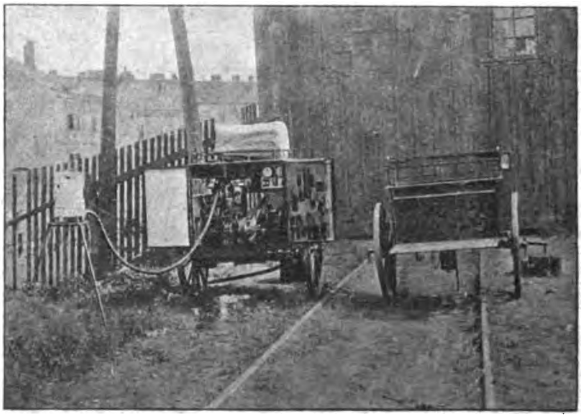


Fig. 1.

methods of its application, it is unfortunately necessary briefly to consider the physical and chemical actions of these substances in water, but I promise to cut this part of the subject as short as possible.

Ordinary alum is a double sulphate of aluminium and potassium (or ammonium, etc.), and when alum is dissolved in water containing a carbonate, aluminium carbonate is first formed; this compound is unstable, and at once reacts with water to form aluminium hydroxide and free carbon dioxide. The formation of this aluminium hydroxide is the essential factor. It is a whitish gelatinous substance which slowly sediments, or can be removed by filtration through sand or fabric. In sedimentation, the hydroxide carries down with it a large percentage (ninety to ninety-eight per cent) of the organisms in the water. Similarly the layer of hydroxide formed by filtration through sand or fabric retains and prevents the passage of practically the same percentage of bacteria originally in the

water. The presence of a carbonate is essential, and, if not naturally present in the water, it must be added with the alum. The clarifying powder used in the Army has the following formula:—

Aluminium sulphate...	...	...	2 parts
Anhydrous sod. carb.	...	...	1 part

and the usual dosage is five grains per gallon.

Bleaching powder is calcium chloro-hypochlorite containing thirty per cent of available chlorine. Its exact reaction in water is still the subject of much controversy, but the probable action is as follows:—

(*a*) In solution bleaching powder breaks up into calcium chloride and calcium hypochlorite.

(*b*) Calcium hypochlorite reacts with carbonic acid (present in all waters) to form hypochlorous acid and calcium carbonate.

(*c*) Hypochlorous acid in water decomposes to give off free chlorine.

(*d*) Free chlorine (in sunlight) decomposes water giving hydrochloric acid and free oxygen.

Free chlorine may form simple unstable chloramines, which would be powerful disinfectants, but disinfectant action may equally be due to hypochlorous acid, free chlorine, or nascent oxygen. Whatever theory may be correct, "free chlorine" is the most convenient factor for estimation.

An important point is that chlorine will attack and oxidize dead organic matter in water in preference to living organisms; the chlorine is said to be "deviated" by the organic matter. The one part per million of free chlorine found necessary for sterilization is what remains available after all chlorine necessary for combination with dead organic material has been removed by such combination. Therefore waters vary in the amount of original free chlorine which has to be added to give a final result of one part per million of free chlorine.

Recognition of this "deviation" effect and the consequent varying chlorine requirements of different waters led during the war to an investigation as to whether there was available a test for the presence of free chlorine, stable, quantitatively definite, and simple enough to be used in the field. The result was the evolution of a test box (fig. 2) based upon a principle originally devised by the late Sir Sims Woodhead, and developed for field service at the Royal Army Medical College under the supervision of Sir William Horrocks. The indicator used in this outfit is a solution of zinc iodide and starch; other indicators were tested during the investigations, but none was found to be so satisfactory. Iodide and starch test-papers reacted only faintly with three to five parts per million of chlorine; aniline required a considerable quantity of the reagent and a strength of some three or four parts per million of chlorine to give its colour reaction; orthotolidine (used as a test in the United States Army) gave its orange-red colour reaction with as little as one part of chlorine in ten million, and

was, in fact, so delicate that traces of atmospheric chlorine tended to vitiate the reaction.

This "case water-testing sterilization," better known as a "Horrocks Box," is the standard Army method for determining the amount of bleaching powder or chlorine required to be added to a water for effective sterilization.

After sterilization of a water by chlorine it is often desirable to remove residual free chlorine; this may be done by any innocuous reducing agent, the chemicals most frequently used being :—

(*a*) Sulphur dioxide.—Final products are hydrochloric and sulphuric acids, giving salts of any bases in the water.

(*b*) Sodium thiosulphate.—Final products are sodium tetrathionate and sodium chloride.

FIG. 2.

Dechlorination may also be effected by free exposure to the air for two hours, by passing the water through vegetable charcoal or through scrap-iron, etc.

The standard Army purification method of "clarification by alum and sterilization by chlorine" can be employed in a variety of ways, which may be summarized as follows :—

(A) Preliminary clarification by alum :—
- (1) Addition of alum (five grains per gallon).
- (2) Formation of aluminium hydroxide.
- (3) Removal of the aluminium hydroxide by: (*a*) sedimentation; (*b*) filtration through sand; (*c*) filtration through fabric; (*d*) combination of (*a*), (*b*), (*c*).

(B) Subsequent sterilization by chlorine :—
- (1) Addition of one part per million free chlorine (allowing for "deviation") in the form of: (*a*) bleaching powder; (*b*) caseous chlorine.

(2) Contact for twenty to thirty minutes.

(3) Dechlorination, as required by: (*a*) sulphur dioxide; (*b*) sodium thiosulphate ("hypo"); (*c*) sodium sulphite.

A study of the various apparatus and installations which were used during the war shows that each of the above methods was employed at one time or another, the choice depending upon the local and other conditions of use. The following may be taken as representative types.

Horrocks Portable Clarifier and Sterilizer.

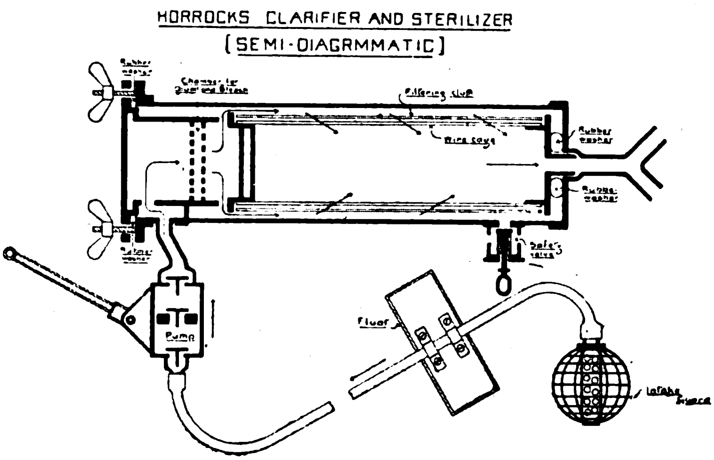


Fig. 3.

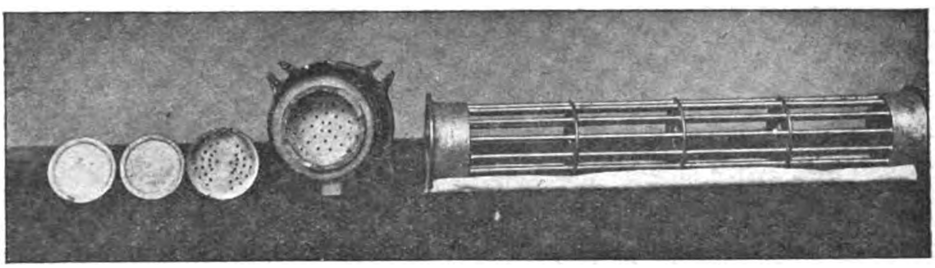


Fig. 4.

The drawing (fig. 3) shows the principle of this apparatus to be the formation of a layer of aluminium hydroxide upon a fabric which is supported on a wire frame, the water being clarified by passing through this filtering medium. Alum powder is placed in the chamber and is taken up by the water in its passage through. Bleaching powder can be added with the alum, if combined clarification and sterilization is desired, but the best results are obtained by using alum alone for clarification and subsequently chlorinating the clarified water in a separate container.

Fig. 4 shows the detailed construction of the alum chamber and the clarifying cylinder, and fig. 5 shows the complete apparatus.

The total weight, including the wooden case, is 150 pounds and the capacity of the apparatus is 100 gallons of clarified water per hour.

The desirability of chlorination subsequent to, and separately from, clarification rendered necessary some form of receptacle for chlorination of the clarified water, and a modification of the apparatus was designed.

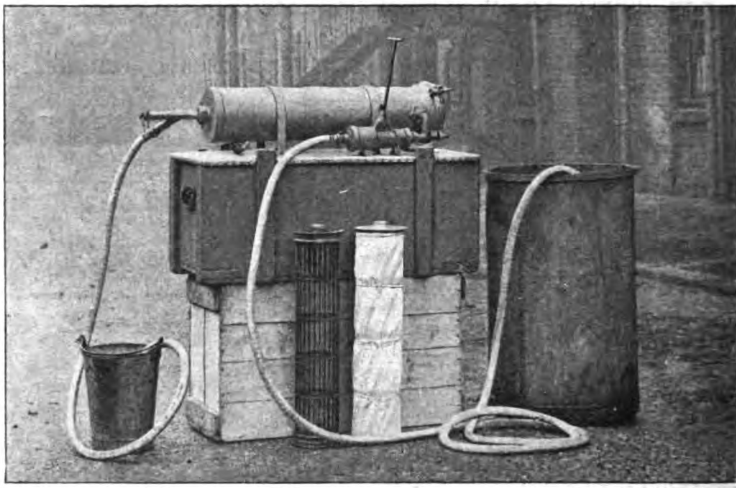


FIG. 5.

FIG. 6.

This included two 100-gallon canvas tanks and the whole was packed in two baskets. The modified apparatus is illustrated in fig. 6, and the attachment for filling water-bottles should be noted.

THE ARMY WATER CART.

This apparatus (fig. 7) is simple in construction compared with the complicated filter cart formerly in use. It is merely two clarifying

cylinders delivering water into a 110-gallon tank in which chlorination is effected by the addition of bleaching powder. The cylinders are the same as in the Horrocks portable apparatus already described. This cart has admirably stood the test of war, and for its purpose (supplying a community of four to five hundred men) could hardly be bettered.

The output of this cart, dealing with the average raw water to be found in the field, is from 150 to 200 gallons per hour.

Fig. 7.

Motor Lorry Sterilizers.

Though the Army water cart was serviceable and adequate for the provision of safe water to individual units, it was soon realized that water must be purified in much larger quantities to meet the requirements of an army in the field. In June, 1915, the provision of special purification plant installed on motor lorries and on barges was recommended for the Expeditionary Force in France. The first motor lorry sterilizers sent to France were designed for sterilization by means of bleaching powder (they were later converted locally into chlorine gas sterilizers), with dechlorination by means of ferrous sulphate. Their output was low, some 400 gallons per hour, but they were in working condition up to the time of the Armistice, and No. 1 Company claims to have delivered no less than 60,000,000 gallons of water for the use of the troops. The next type of lorry used chlorine gas for sterilization, and this modification, along with

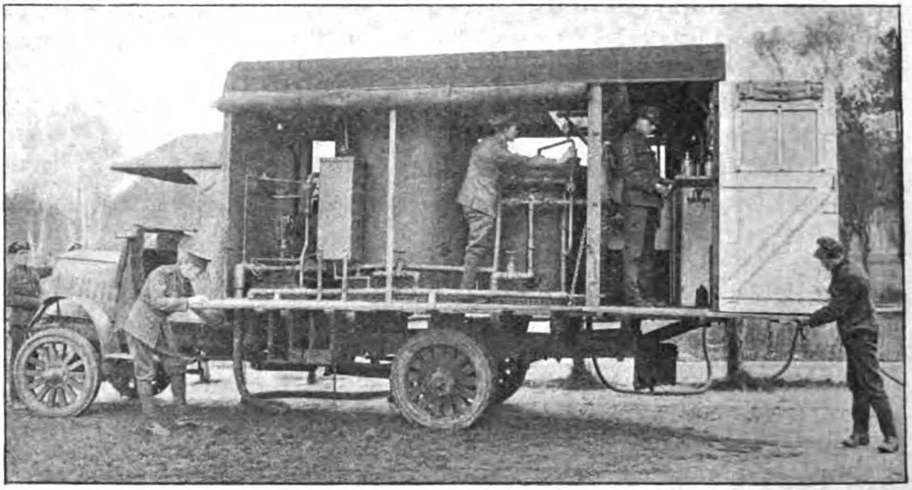


FIG. 8.

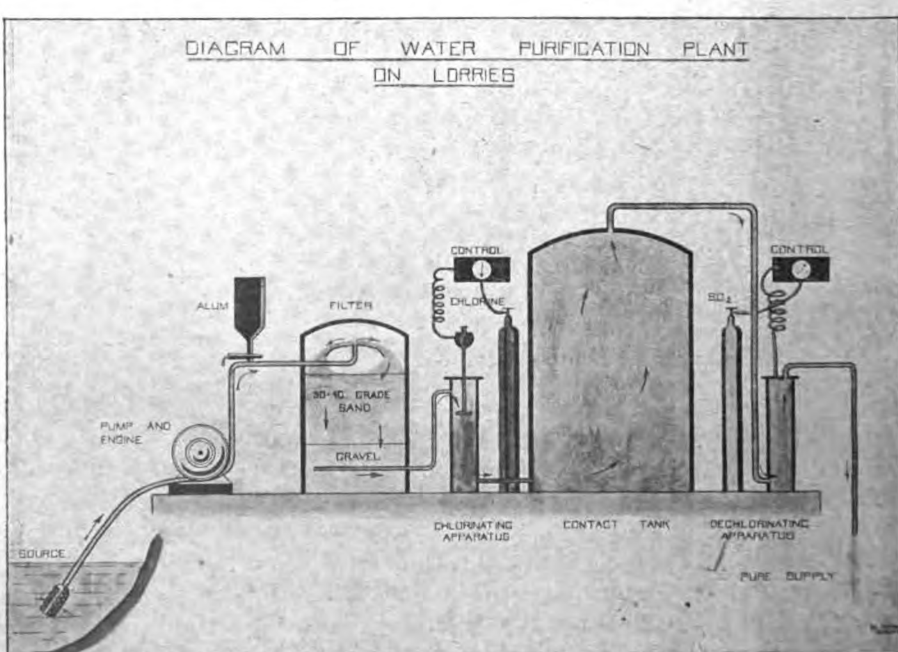


DIAGRAM OF WATER PURIFICATION PLANT
ON LORRIES
CONTROL
CONTROL
ALUM
FILTER
CHLORINE
SO_2
30-40 GRADE
SAND
PUMP AND
ENGINE
GRAVEL
SOURCE
CHLORINATING
APPARATUS
CONTACT TANK
DECHLORINATING
APPARATUS
PURE SUPPLY

FIG. 9.

other improvements, raised the normal output to 1,200 gallons per hour with the ability to increase the output to over 2,000 gallons per hour for short periods. This type of lorry is illustrated in fig. 8, and these plants played so important a part in the supply of safe water during the war, and will play no less important a part in any future campaign, that I make no apology for describing them in detail.

Their working will be best understood from the diagram in fig. 9.

By means of a pump, driven by a small petrol motor, water is pumped from the source of supply to a sand filter, being dosed with alum solution *en route* by a pipe from the alum reservoir. From the sand filter the clarified water passes through a small cylindrical chamber, where it is chlorinated. Chlorination is effected by chlorine gas which, stored under pressure in a steel cylinder, passes through a control apparatus and comes into contact with the onflowing water in the chlorinating chamber. By means of a diffuser, the chlorine gas reaches the water in so minute bubbles that complete and immediate solution results. From the chlorinating chamber the chlorinated water passes to a large closed tank of such a size that an average period of contact sufficient for sterilization is ensured. From this contact tank the chlorinated water passes to a second small cylindrical chamber where it is dechlorinated by means of gaseous sulphur dioxide. The sulphur dioxide, stored under pressure in a steel cylinder, is mixed with the water by means of a diffuser similar to that used for chlorination. The water clarified, sterilized, dechlorinated, emerges ready for consumption from an exit pipe.

Similar plant, having an output of 4,000 gallons per hour and designed also for the removal of poisons, was installed upon barges for use on the canals in Flanders, still larger plant, with an output of 8,000 gallons per hour, was placed on barges and used on the rivers in Mesopotamia, and fixed plant with an even larger output was also installed.

There is no doubt that for installations of a large or moderate size, and even for quite small ones if they are in constant use, sterilization by gaseous chlorine with dechlorination by sulphur dioxide is the best, the most efficient and the most economical method. Bleaching powder will never be entirely replaced by chlorine gas; it still remains the best sterilizing agent for small outfits, especially if not in continuous use. It would be futile, for example, to think of adopting gas sterilization in place of bleaching powder for regimental water carts, while for improvized installations bleaching powder furnishes the only possible method.

Improvized Portable Installations.

The principles underlying the standard types of apparatus, which have been described, can be utilized in the construction of emergency installations, and these can be improvized in the simplest manner from such

material as is always available in the field. Thus, in its simplest form, the installation shown in fig. 10 is actually only two canvas tanks and a hand pump.

One tank is a sedimentation tank in which the water is treated with alum and allowed to sediment, the supernatant clear water is then drawn

DIAGRAM SHOWING PORTABLE ALUM SEDIMENTATION AND CHLORINATING INSTALLATION.

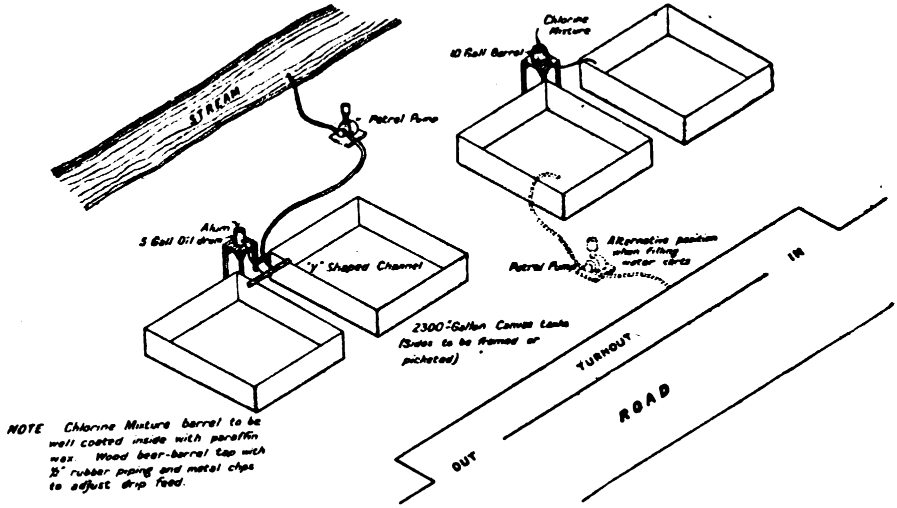


FIG. 10.

ROUGH SKETCH OF CHLORINATING SET FOR BOTTLE FILLING.

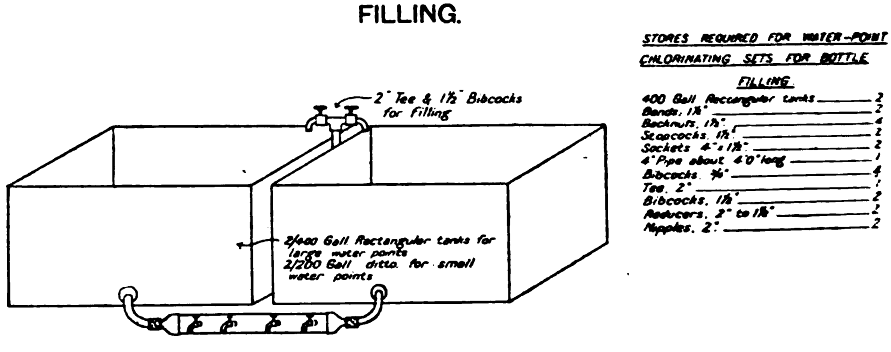


FIG. 11.

off into the second tank, dosed with bleaching powder solution and distributed from the tank after the required period of contact.

If only chlorination, without clarification, is required, the arrangement is even simpler, as shown in fig. 11, in which the arrangement of taps for filling water-bottles should be noted.

WATER TANK.

WOOD, WIRE-MESH and CANVAS.

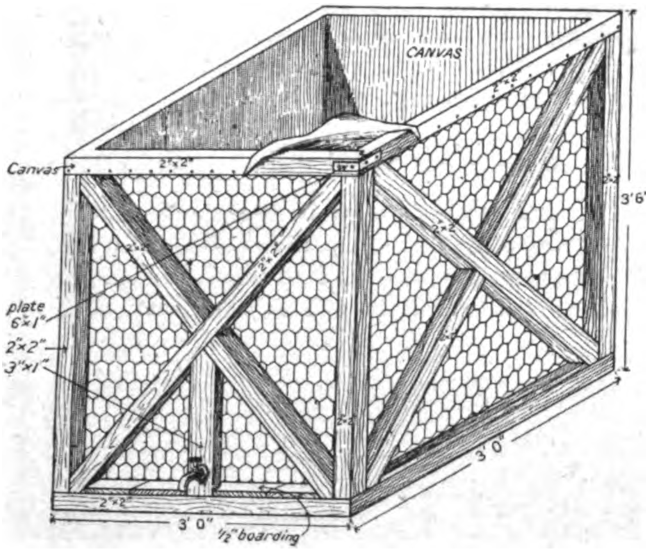


FIG. 12.

LYSTER BAG FOR WATER STERILIZATION

FIG. 13.

In each of these examples double tanks are employed to permit of continuous working, and the tanks themselves can easily be improvized, as illustrated in fig. 12.

Similar to these improvized appliances is the Lyster bag, used in the United States Army for sterilization of water by bleaching powder. The bleaching powder is supplied packed in glass tubes, each containing one "dose" for a bagful of water (fig. 13).

Improvized Fixed Installations.

The principles and methods used in the standard wheeled outfits and the improvized portable plants are equally applicable to fixed installations. In the various war areas these were of all sizes, varying from a small unit of a couple of tanks (sedimentation and chlorination) and a pump, to large

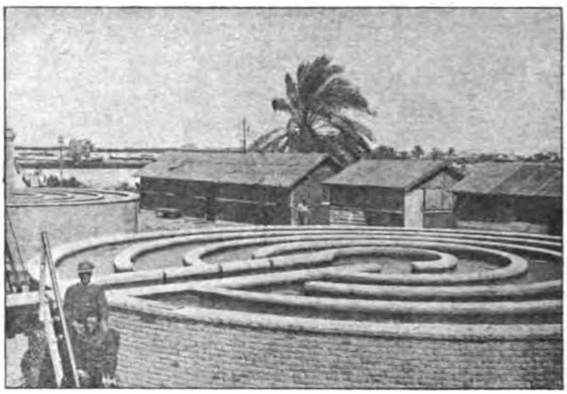


Fig. 14.

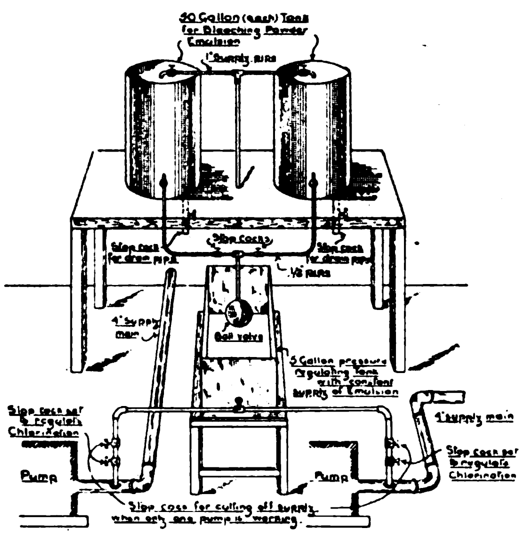
plants using sand or mechanical filters for clarification (with or without a preliminary sedimentation), and, in the majority of cases, using bleaching powder and not gas for sterilization. The amount of water dealt with in these plants reached 20,000 gallons per hour in some cases. The settling, chlorinating, and storage tanks in many instances were merely canvas tanks or dugout tanks lined with tarpaulins. In some cases (e.g., Kantara Water Works, Egypt) the sedimentation tank was replaced by a sedimentation labyrinth (fig. 14), through which the water flowed at a slow rate, depositing the alum precipitate. In this case the effluent from the labyrinth was passed through mechanical filters, and it will usually be found that the effluent from an alum sedimentation tank still contains sufficient aluminium hydroxide to form a satisfactory film for subsequent sand filtration.

Where chlorine gas is used in such fixed installations the dosage is measured and controlled by some standard type of chlorinator, as in the motor lorries or barges. In most cases, however, bleaching powder is used

and is best added to the water, as it flows into the chlorinating tank, by some form of constant feed apparatus. For this purpose an apparatus delivering bleaching powder in the form of a strong watery emulsion is much more satisfactory than any apparatus designed to deal with the dry powder, and two types easily improvized from ordinary army material deserve notice.

In both the principle is the same. A strong emulsion of bleaching powder (a convenient strength is one four-ounce tin to eight gallons of water) is made in a container from which a pipe passes to a constant level

IMPROVISED
AUTOMATIC CHLORINATING PLANT



DIAGRAMMATIC SKETCH

FIG. 15.

chamber. From the constant level chamber the emulsion passes by a feed pipe to mix with the water flowing into the chlorinating tank, the feed pipe being provided with some means for regulating its flow. The rate of flow is measured by the time required to fill an eight-ounce measure, thus with a water requiring two scoopfuls of bleaching powder per water cart and flowing at the rate of 2,000 gallons per hour, the dosage of bleaching powder emulsion of the above strength is correct when the flow from the feed pipe fills an eight-ounce measure in forty-five seconds. To avoid calculations tables based upon the above factors can be constructed to show

at a glance what should be the rate of flow from the feed pipe for any rate of water flow and for any chlorine dosage required.

Fig. 15 shows one form of such an apparatus in which the constant level chamber is controlled by an ordinary ball-cock, the rate of flow of the water is gauged from the rate of working of the pumps, and the dosage of bleaching powder emulsion is regulated by a stop-cock on the feed pipe.

In fig. 16 is illustrated a simpler form of chlorinator in which the ball-cock is replaced by an ingenious cork float controlling a valve, the quantity

ADJUSTABLE CONSTANT FEED CHLORINATOR

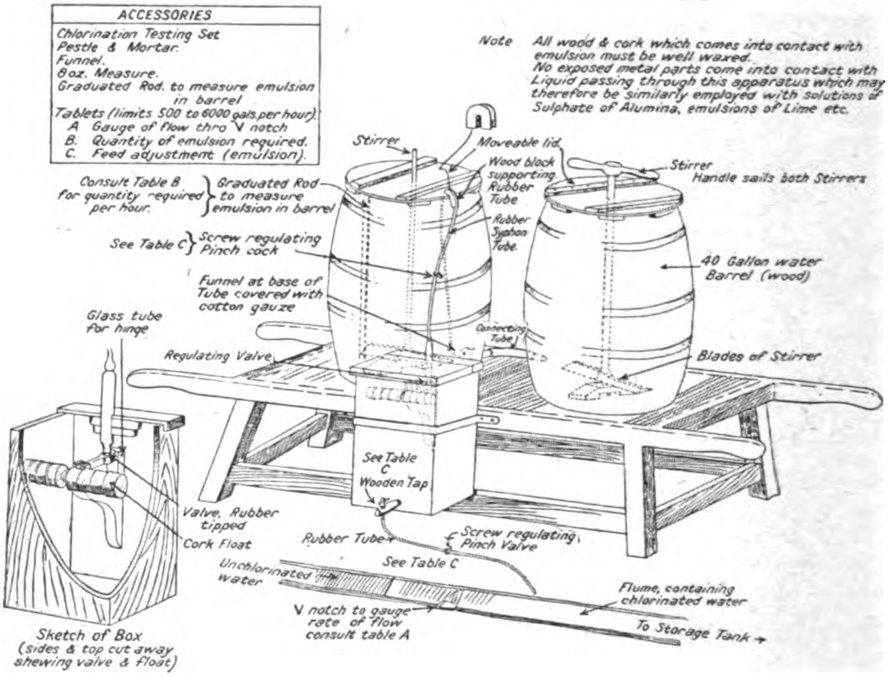


FIG. 16.

of water is measured by its rate of flow in a V-shaped flume, and the dosage of emulsion is controlled by a screw pinch-cock on a rubber feed pipe.

SOME GENERAL CONSIDERATIONS IN REGARD TO CHLORINATION.

Even with apparently clear waters it is advisable to carry out alum clarification before chlorination. It must be remembered that chlorine has a low power of penetration into the interior of solid infective material, and, with the ordinary period of contact, simple chlorination cannot be expected to sterilize a water containing, for example, flakes of infected mucus or particles of infected excreta. Further, clarification may lessen

the required dosage of bleaching powder by removing suspended organic matter which would otherwise "deviate" part of the available chlorine.

Under active service conditions complete dechlorination of the finally treated water is seldom advisable, and a certain amount of residual chlorine (one-third to one-half part per million) is desirable in view of the possibility of subsequent contamination during distribution or storage. When one considers what improvized vehicles and containers are frequently used for the transport or storage of sterilized water, the possibilities of contamination after sterilization are obvious and a small amount of residual free chlorine is a necessary precaution.

It is good policy to effect sterilization as far forward as possible. Thus, a water point may supply only clarified water, chlorination being done in the water-carts, and a notice at the water point states the number of scoopfuls of bleaching powder required for each water-cart. Similarly the standard practice in France was to avoid chlorination at the source of supply. At the same time chlorination at the source may sometimes be necessary. Thus when troops are advancing over a wide front dotted with fouled wells or water holes, chlorination of the wells themselves may be organized. Wells which have been chlorinated may be marked by throwing into the water a handful of waxed or greased confetti, a method used in the Egyptian Expeditionary Force.

Against the cheapness of bleaching power, its simplicity in working, and its general suitability for water sterilization, must be placed its serious disadvantage that the substance is very easily decomposed by exposure to even a moderate temperature or by prolonged storage even in hermetically sealed containers. This decomposition is due to the presence of moisture in the manufactured product, but removal of this moisture is extremely difficult, as sufficient heating to drive off the moisture drives off also the whole of the available chlorine. Chlorine gas is free from this disadvantage, but, as already stated, gas can never replace bleaching powder in all the varieties of purification plant.

A large number of suggested substitutes have been tried, and amongst these may be mentioned "brom-bleach" (chlorine replaced by bromine), a product sufficiently thermostable to permit of the contained moisture being driven off by heat. Eupad and eusol have been tried and the wide use of chloramines in surgical practice suggested the possibility of using these very stable chlorine compounds for sterilization of water. From time to time other chlorine compounds and solutions have been introduced for which marked stability has been claimed. Speaking generally, if such substitutes are satisfactory they are also expensive, and those which are cheap prove on trial to be little more stable than plain bleaching powder. In Mesopotamia an endeavour was made to overcome the difficulty by testing the bleaching powder in a laboratory and making from it standardized emulsions or chlorine solutions. These liquids were then bottled and issued with instructions that one bottle would sterilize a stated number of

gallons of water. Mention may also be made of the practice in the French Army, where chlorine was used for water purification in the form of Eau de Javelle, a clear solution of sodium hypochlorite.

In the direction of obtaining a stable bleaching powder a large amount of work [1] has recently been done at the Royal Army Medical College with excellent results. It was found that a mixture of one part of quicklime with four parts of bleaching powder, if properly packed, maintained its original chlorine strength even after prolonged storage under tropical conditions. The initial reduction in available chlorine due to dilution of the bleaching powder with quicklime is more than compensated for by the greatly increased stability of the mixture, and the formula has been adopted for future army supplies.

It would be impossible to discuss the purification of water by means of chlorine without at least mentioning the universally known—and as universally disliked—chlorine taste. Many attempts have been made to explain this phenomenon, but no generally satisfactory reason has yet been advanced. Where over-chlorination exists, the cause and the remedy are simple, but this explanation meets only a small fraction of cases. The action of chlorine on organic matter in water forming simple chloramines or chlorphenols has been suggested, but the defect is not always most marked with highly polluted waters. The solvent action of chlorine upon metal containers, especially those made of zinc or galvanized iron, has recently been suggested [2] as an explanation supported by a fair amount of evidence. Opinions seem to agree that the unpleasant flavour is less marked where chlorination has been done by gas than where it has been done by bleaching powder. There we must leave the problem for the present until further investigation produces sufficient evidence to justify a definite opinion.

Individual Methods of Water Purification.

Up to this point we have been considering methods of clarification and sterilization suitable for bodies of men numbering at least half a battalion (one water-cart) or of sufficient size to justify the transport weight (150 pounds) of a Horrocks clarifier. For smaller detached parties there exist no standard arrangements for water purification, while at the same time these are the very people who are most likely to take their supply from any casual source, and hence to run most danger of getting thoroughly bad water. Any methods suggested for the use of small parties should be such as can also be made applicable to single individuals.

For individual water purification small pocket filters have been suggested, and during the war these were usually found in the kit of the innocent subaltern on his first departure overseas. These small filters require just as meticulous care and attention as do the larger ones, they furnish added weight to the soldier's load, and they need not be further considered.

Boiling of water can of course be carried out by individuals but must be regarded as a purely emergency method.

Therefore, we have to rely upon some chemical agent as a solution of the problem, and of such methods two deserve mention: the three-tablet iodine method originally introduced by Vaillard, and bisulphate of soda tablets first advocated by Rideal and Parkes and modified by Firth.

In the Vaillard method three tablets are used: No. 1 (blue) contains potassium iodide and sodium iodate; No. 2 (red) contains tartaric or citric acid; No. 3 (white) contains sodium thiosulphate. If a blue and a red tablet be crushed and dissolved in a little water a solution of iodine is obtained, and this added to the contents of a water bottle gives six parts of iodine per million. After ten to twenty minutes contact a white tablet is added which neutralizes the iodine. It is an ingenious and effective method, but its complexity condemns it for general use; in many cases the tablets are added in the wrong order or sufficient time for contact is not allowed.

In the Rideal and Parkes method, tablets (two grammes) of bisulphate of soda are flavoured with saccharine and oil of lemon. One tablet dissolved in the contents of a water-bottle gives an acid solution with a flavour faintly resembling lemonade. The solution is really a weak (0·07 per cent) solution of sulphuric acid. This is the present army method for individual water sterilization, but it has many disadvantages. The solution is powerfully metal-solvent and produces a most objectionable taste on prolonged contact with the aluminium water-bottle. In the case of water-bottles made of copper, such as those issued with medical equipment, the use of an acid metal-solvent solution is obviously inadvisable. Water-bottles made of enamelled iron are safe if the enamel is intact, but if the enamel is chipped the exposed iron of the bottle corrodes rapidly and ferrous sulphate is formed, giving an objectionable taste and colour to the contents. It has also been observed that when these tablets come into contact with moisture—even if it is only the moisture of the atmosphere—enough sulphuric acid is liberated to destroy clothing or to burn the skin, and as they are issued in glass bottles, the risk of such accidents is by no means remote.

Bromine contained in glass capsules (Schömberg's method) has been used, but the disadvantages of fragile glass containers is obvious and the method cannot be regarded as suitable for field use.

As emergency methods it should not be forgotten that iodine (five drops of tinct. iodi mitis to a litre of water) and potassium permanganate are to be found in most articles of field medical equipment, and even acidifying the water with hydrochloric or sulphuric acid is better than nothing.

The success obtained by the use of chlorine for bulk sterilization naturally suggests its use also for individual purposes, and Lelean recommends the following ingenious method. If one scoopful of bleaching powder be mixed with the contents of one water-bottle to form a strong

emulsion, and one scoopful of this strong emulsion be added to the contents of another water-bottle, the resulting dilution will contain one part per million of free chlorine. By increasing the number of scoopfuls for the first bottle waters with varying degrees of pollution may be sterilized. The aromatic chloramines as a source of chlorine for individual water sterilization were investigated by Dakin [3] during the war, but they proved unsatisfactory. More success was obtained with a compound of a slightly different nature to which the name of "Halozone" was given. The necessity for individual sterilization is usually of short duration and the issue of small bottles of standardized bleaching powder emulsion, of sodium hypochlorite solution, of eusol or of some other stable chlorine solution has been suggested.

On the whole it may be said that a satisfactory method for the individual sterilization of drinking water has yet to be evolved, but the proved keeping qualities of the new lime-bleach mixture lead one to hope that small tablets of this product, one being sufficient to sterilize the contents of a water-bottle, will prove the future solution of this problem.

Special Purification Processes.

The purification of water has been discussed at some length in regard to clarification and sterilization, as these two processes are at once of supreme importance and of almost universal necessity. Under special conditions, however, there may arise, and did arise during the war, the need for special processes of purification, and some of these may now be briefly mentioned.

Removal of Poisons.—Depoisoning apparatus mounted on motor lorries was sent out to France owing to the possibility that intentional poisoning of water supplies might be adopted by the enemy. The principles of construction were simple, the poisons being converted into an insoluble form by means of appropriate chemical reagents and the precipitates removed by filtration through sand. These lorries were never used for their original purpose but later proved useful for ordinary chlorination work. The possibility of poisoned water supplies was also responsible for the introduction of a small test case for the rapid detection of certain poisons (arsenic, antimony, mercury, lead, copper and cyanides) in a water sample in the field.

Saline Water.—Where sea or brackish water forms a source of supply, distillation is the only possible form of purification. Both mobile and fixed distilling plants were used on various fronts during the war (Egypt, Gallipoli, etc.), and the improvized distilling plant illustrated in fig. 17 is of interest.

Removal of Sand.—In Egypt and in certain areas in France the presence of very fine sand in water, especially from boreholes, interfered with purification by delaying sedimentation. In Egypt fine wire gauze

was used as a barrier and in France the Ashford sand strainer [4] was brought into use towards the end of the war.

Anti-mosquito Work.—Oiling the water surface of possible mosquito-breeding areas is a recognized anti-mosquito measure, but where the water is also the source of a drinking supply, oiling leads to many complaints. In Macedonia the use of cresol in place of oil was introduced with success, a concentration of 1 in 80,000 being effective against mosquito larvæ and scarcely detectable by taste.

Schistosomata.—The presence of these parasites in Egypt and other parts of Africa necessitated special measures of water purification, and the findings of the Bilharzia mission in Egypt in 1915 furnished a scientific basis on which to work. Cresoled water (1 in 10,000) was used for wash-

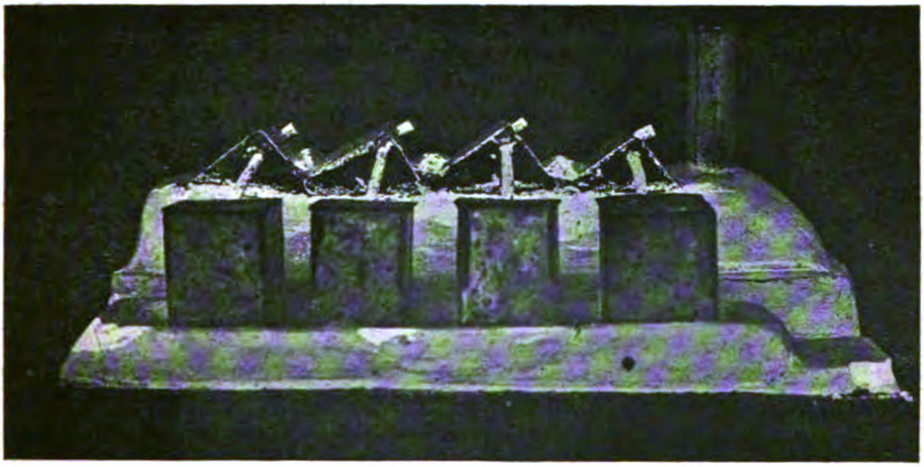


Fig. 17.

ing; drinking water intakes were screened with wire gauze (sixteen holes to the inch) to exclude the snail hosts of the parasites, and drinking water was either specially chlorinated (two parts per million) and subsequently dechlorinated, or stored for forty-eight hours after ordinary chlorination.

Leeches.—Tiny active leeches were prevalent in many water sources in tropical and semi-tropical areas and occasionally caused trouble by lodging and growing in the pharynx. One specimen, over three inches long, was removed from the throat of a British soldier in Egypt. These parasites can be excluded from a drinking water supply by straining through wire gauze of twenty holes to the inch.

Conclusion.

I have endeavoured to show to-night how, starting from original measures inadequate for a campaign such as the recent war, there was built up—with the Horrocks clarifier and test box as a basis—a successful,

extensive and diverse system of water purification. Successful in that throughout the war there occurred no waterborne epidemic of importance; extensive in that the installations varied from mobile apparatus with an output of fifty gallons per hour to fixed plants furnishing up to 20,000 gallons per hour; and diverse in that protection had to be afforded not only against bacterial organisms of disease, but also against poisons, worms, leeches and mosquitoes.

Nevertheless it is safer to refrain from any dogmatic expression of finality. Nothing impressed me more when I was verifying facts and references for this paper, than the universal condemnation, in not very ancient textbooks, of all chemical methods for the sterilization of water in the field. "Fantastical," "Visionary and impracticable," "Excellent in theory but fails in practice," "Incompatible with active hostilities," are but a few of the epithets flung at proposals for the chemical sterilization of drinking water on active service; and when a candidate, in a promotion examination shortly before the war, suggested chlorinating water under battalion arrangements, the examiner retorted that "this would only be suitable and practicable in standing camps and then for short periods only." With such examples before me I hesitate to claim that our present system cannot be bettered, but I do not hesitate to claim that, within the limits of our present knowledge, we now possess a system at once extremely simple and thoroughly efficacious, which may be summed up in the brief phrase which I have mentioned once or twice already: "Clarify by alum. Sterilize by chlorine."

REFERENCES.

[1] AUMONIER and ELLIOTT. JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, xxxix, 459.
[2] LOTHIAN and WARD. *Ibid.*, xxxix, 163.
[3] DAKIN and DUNHAM. *Brit. Med. Journ.*, 1917, 682.
[4] *Military Engineering* (1922), vi, 101.

Figs. 10, 11, 12 and 16 are taken from "Work of the R.E. in the European War, 1914-19, Water Supply—France," by permission of the R.E. Institute.

ARMY HYGIENE ADVISORY COMMITTEE REPORT No. 2.

An Investigation on the Motion Study of Digging and the Energy Expenditure involved, with the Object of increasing Efficiency of Output and economizing Energy.

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AND
Captain R. L. BROWN.
Royal Engineers.

(*Continued from p.* 349.)

SECTION II.

EXPERIMENTAL WORK ON RATE, STYLE AND METHODS OF PICKING AND SHOVELLING.

Records of Experiments.

Subsection I—Shovelling.

(*A*) *Rate of Throw: R.E. Shovel.*

Earth loose, medium type, dry. Smooth cement base. Throw: 5 feet horizontal by 4 feet 6 inches vertical.

For these rate experiments all shovelling was done to the beat of a metronome. The soil was thrown over a definite vertical height at a definite horizontal distance and collected and measured in a box of known cubic capacity, the time of actual filling of the box being taken with a stop-watch. Table IV shows the detailed results of shovelling with the R.E. shovel and is given as a sample table of work under this section.

(1) *Without aids.*—All members of the squad were tried in rates varying from fourteen throws per minute to twenty-four throws per minute and an optimum average in relation to output and rhythm was obtained

This optimum fell between nineteen and twenty throws per minute. (See Table IV.)

No aids (e.g., using knees) were used in this shovelling, no member appearing to find the necessity.

(2) *With aids.*—Under the same conditions of soil, base, height and length of throw, aids were employed, the rear knee being used as an additional force to help the forward thrust into the material (also the forward knee to a lesser extent). The rate of the throw was reduced by this and fell to seventeen throws per minute as the optimum for rhythm and output.

TABLE IV.—SHOVELLING (WITHOUT AIDS). RESULTS OF WORK UNDER SECTION 2.

To Show Relation of Rate of Throw to Output and Rhythm.

Tools: *R.E. Shovel.* Earth: Medium type. Base: Smooth cement. Throw: 4 feet 8 inches vertical by 5 feet horizontal. Time (in seconds) taken for subjects to shovel 1,580 lb. of earth (18 cubic feet loose).

Throws per minute	Subject								Remarks
	"D"	"B"	"C"	"F"	"E"	Average time for 8 c.f.	Average output in lb. per minute	Average shovel load in lb.	
14	..	..	..	..	..	..	..	..	Co-ordinated action impossible
15	575	558	572	573	545	251	162·7	10·85	Too slow. No co-ordination
16	440	453	480	502	453	207	197·5	12·35	Still too slow
17	338	375	396	422	400	171·5	238	14	Slow
18	350	368	410	410	383	170·7	239	13·28	Rhythm better. Slow
19	320	388	326	435	333	160	255	13·63	Good rhythm } Slow for one man
20	315	362	320	381	322	149·3	273·5	13·78	Good rhythm } Slow for one man
21	317	373	324	360	326	151	270·5	12·89	Good rhythm: fast for one man
22	312	391	332	381	389·5	160	254·5	11·57	A little fast
23	391	446	412	432	469	191	213·5	9·29	Too fast
24	..	..	..	..	..	..	..	..	Impossible
	REPEATED FOR THE BEST FOUR RATES (EIGHT CUBIC FEET).								
18	143	193	170	190	169	173	236	13·11	Rather slow
19	135	185	150	185	165	164	249	13·11	Good rhythm } Appears to be optimum rate
20	122	149	151·5	167·5	165	151	270	13·52	Good rhythm } Appears to be optimum rate
21	125	160	160	160	162	154	265	12·62	Rather fast

(B) *Rate of Throw: G.S. Shovel.*

Experiments were conducted as above.

(1) *Without aids.*—Twenty-one throws per minute was the optimum rate.

(2) *With aids.*—Was not investigated, being entirely unnecessary with so small a shovel.

(C) *Variation in Methods: R.E. Shovel.*

Throw, base, and soil as before. Rate nineteen per minute as a standard. After experimenting on all these subjects we came to the following conclusions:—

The best position and methods for a right-handed shoveller appear to be as follows:—

(1) *Feet.*—Left foot pointing directly to material, toe within 2 inches of it. Right foot behind and to right of left. Distance from centre of heel to centre of heel 12 inches to 15 inches. Angle formed by feet 80° to 90°.

This gives a good base and helps the body balance. Feet should be kept still during work except to advance as material is thrown out.

(2) *Hands.*—Right hand grips the T-piece, thumb round. Left hand grips the shaft at the bend near the blade, palm uppermost. Grip loose. For a short throw, i.e., 3 feet and below, left hand only moves an inch or

two. For a long throw, i.e., over 3 feet, the left hand slides up the shaft up to 9 inches thus relieving the strain on the muscles of the upper arm in throw.

(3) *Balance.*—At first the balance is evenly distributed on both feet. To commence shovelling the weight is first put on the rear foot as the arms swing back, then as the shovel is swung forward into the soil the weight is transferred to the left foot, again returning to the right foot as the load is swung back and once again to the left foot as the throw is made.

(4) *Position of Body.*—Both knees are slightly flexed and the position is one of perfect ease. Back is bent, left arm is slightly flexed and muscles loose, right arm flexed to about a right angle and loose. Shoulders thrown forward.

(5) *Motion.*—Take up position as described above. Swing arms back, transferring weight to right foot then forward bringing the body weight through the knees to the left foot, the face of the shovel going horizontally into the material by sliding along the base. At the same time depending on the nature of the soil and of the base (e.g., heavy soil and rough base), back up this forward thrust by pressing the inside of the right knee on the right hand and the left knee on the left hand by crouching and bringing whole weight forward through the knees.

The shovel having been filled, *depress the T-piece by a slight downward movement* of the right hand, at the same time swinging back with a change of weight to the right foot and without pausing swinging forward again, shifting the balance and throwing the material to the required spot. The swing back and forward should be of a pendulum type, the forward hand sliding the necessary distance in the forward throw. In the throw the back is only partially straightened depending on the height of throw. This completes the full movement.

(6) *Rhythm.*—The part played by rhythm is a most important factor. The body has a natural swing. All muscle forces are brought into play without any jerking or interruption and these forces acting in conjunction produce the necessary movements with relatively little expenditure of energy. A good swing or rhythm at an easy natural pace can be kept up for a long spell without undue fatigue.

(7) *Changes in Position.*—During a long spell, to rest some muscle groups and to bring others into more active play, change over and shovel left-handed. This may be decidedly awkward at first, but practice and attention to the above details (with hand and feet positions reversed) will produce rapid efficiency.

Again the grip of the forward hand may be reversed as an additional rest for the flexors of the upper arm and the extensors brought into concentric action, thus producing a complementary action and relieving muscle fatigue.

(8) *Length of Spell.*—The optimum length of spell appears to be about two minutes.

(9) *Other Factors in General Methods.*—(*a*) The shovel pan should always be thrust below the loosened earth and not into it. Thus the keeping of a smooth trench base is essential.

(*b*) The shovel pan should never hit or be rested on the trench parapet during shovelling.

(*c*) Loose earth *alone* should be scraped together at the end of a shovelling spell and no attempt should be made to cut into or move unloosened soil.

(*D*) *G.S. Shovel.*

Methods of action are identical to those described for the R.E. shovel, the rate being a little faster (two throws a minute increase), i.e., twenty-one per minute.

PRACTICAL EXPERIMENTS.

(1) INFLUENCE OF HEIGHT AND LENGTH OF THROW ON RATE, OUTPUT AND SHOVEL LOAD.

How much the factors of speed and output were influenced by varying the horizontal and vertical throws was tested on practical lines on different subjects.

Tools, soil and general conditions were standardized for all. Height of throw was varied a foot at a time by using a chess framework, and the earth thrown over was collected and accurately measured in a box of known dimensions. Horizontal throw was measured from where the soil was originally lifted to the four foot-high chess over which it was thrown.

Consideration of Results.

(*a*) *Vertical throws.*—Initial tests showed that in vertical throws the rate of shovelling did not vary to any marked extent, so it was considered that more value would accrue from these tests if definite rates were adhered to. Those chosen were nineteen and twenty-one per minute.

Up to a height of 4 feet 3 inches little change in shovel-load and output was noticed, except that a better result was obtained at 4 feet 3 inches than at 3 feet 3 inches. Up to these heights there is little actual lift needed and the back does not straighten to any extent. Above 4 feet 3 inches there is a proportionate decrease in output and shovel-load. For the optimum rate of nineteen per minute the percentage decrease in output from 4 feet 3 inches to 7 feet 3 inches was as follows:—

Average output at 4 feet 3 inches					...	= 226·5	pounds	per minute
6·5 per cent decrease		at 5 feet	3 inches		...	= 211·8	,,	,,
9·0	,,	,,	at 6 ,,	3 ,,	...	= 206·2	,,	,,
18·5	,,	,,	at 7 ,,	3 ,,	...	= 184·7	,,	,,

At the rate of twenty-one throws to the minute the percentage decrease in output was as follows:—

Average output at 4 feet 3 inches	...	= 274 pounds per minute	
9·5 per cent decrease at 5 feet 3 inches	...	= 248	„ „
18·2 „ „ at 6 „ 3 „	...	= 229·5	„ „
30·1 „ „ at 7 „ 3 „	...	= 191·5	„ „

There is, therefore, a definite progressive decrease in output from 4 feet 3 inches upwards, especially noticeable above a throw of 6 feet high. The quicker the rate of throw the greater the decrease of output above 4 feet 3 inches.

(*b*) *Horizontal throws.*—Results show that up to a throw of 4 feet there is little change in rate, output, or shovel-load. After 4 feet there is a progressive decrease in all these factors as follows :—

	Average rate	Average output	Average shovel-load
Average shows ..	20·4 per minute ..	224·8 pounds ..	11·02 pounds at 4 foot
Percentage decrease of	1·96 per cent ..	15·9 per cent ..	14·2 per cent at 6 foot
„ „	7·75 „ ..	28·8 „ ..	23·1 „ „ 8 „
„ „	11·96 „ ..	36·1 „ ..	27·5 „ „ 10 „
„ „	14·70 „ ..	42·3 „ ..	32·8 „ „ 12 „
„ „	20·1 „ ..	52·4 „ ..	40·5 „ „ 14 „

(2) The Influence of Right- and Left-handed Shovelling on Output and Shovel Load.

A small test experiment was done on the above, and the averages of two subjects taken.

The result of this test showed that there is little variation in the two methods, and this being so, a training which will include throwing both right-handed and left-handed would be of great value, as the two methods afford a change and rest for each other, and favour throwing out the soil on different sides of the trench.

Subsection II.—Picking.

Picking.—Tool: Pick $4\frac{1}{2}$ pound head. Earth: Medium type—dry.

Various experiments were carried out, using the same methods as those employed under shovelling, to define the optimum rates and styles of picking, and the results obtained are considered below.

Consideration of Results.

(1) *Rate of Stroke.*—This is a variable factor depending on the nature of the soil, and no definite rate can be laid down. In general, a rate of between twenty-five to thirty strokes per minute appeared the best.

(2) *Raking.*—These short strokes (previously described) would appear to be better combined in one full pick action than made a separate motion in themselves.

Thus, after the pick-head hits the ground and the small end of the helve is raised and pushed away to break the soil, a raking movement should be superimposed on this action by advancing the forward hand as the helve is raised and afterwards drawing the point of the pick-head through the loosened soil, thus doing away with one set of actions in combining the two movements.

In hard brittle soil this combined raking stroke is not so essential to efficiency as in a soil which breaks away in large clods.

(3) *Length of Spell.*—The optimum picking spell would appear to be about 100 seconds, but this period will naturally vary with different types of soil and on varying lengths of face worked. Thus, on a light soil, half that period would suffice, and in general one can lay down the rule that when, without advancing the feet, sufficient earth has been loosened and raked down, so that any further picking will only affect already loosened earth and not break up any further solid earth, the spell should be discontinued.

(4) *Position of Feet.*—As for shovelling, except that the feet are a little closer together and the angle formed is slightly less. Position when once taken up should not be changed, and earth should be raked down on the forward feet.

(5) *Position and Movement of Hands.*—(*a*) The *rear hand* grips the helve at the extreme end sufficiently tight to hold the pick, and does not move from this position in action.

(*b*) The forward hand grips the helve loosely near the pick-head, the helve sliding through this hand in the downward stroke until at the moment of impact the hands closely approximate, and by gripping tightly convey the force from the body to the pick-head, thus assisting the actual weight of the pick-head in breaking the soil.

(6) *Balance.*—At first the balance is evenly distributed on both feet. As the pick-head is raised, the weight is transferred to the rear foot, and remains there until the top of the stroke is reached. During the downward stroke it is gradually brought forward and conveyed through the hands to the pick-helve. During the raking stroke the weight again reverts from the forward to the rear foot.

(7) *Position of Body.*—Before the commencement of the stroke, knees and arms are loosely flexed. As the pick is raised, the knees are straightened, and the whole body is brought to an upright position as the top of the swing is reached, at which point neither the centre of the pick-head nor that of the body should be behind a vertical line drawn through the centre of the foot base. As the downward stroke is made, the body is gradually inclined forward and downwards, the arms are both fully straightened (particularly the forward arm), the advanced knee is flexed, the rear leg being kept straightened, and the body momentum is combined with the weight of the pick-head to assist in breaking the soil.

(8) *Motion.*—Take up position of hands, feet, and balance as described. Carry the pick-head upwards till the top of the swing is reached, when, without pausing, it is brought forward and downwards to hit a selected spot on the ground. (Detailed actions of hands, body, etc., described above.)

Raise the small end of the helve to loosen the earth by a lift of the hands and slide the forward hand up the helve to the first position. Rake down the loosened earth by a pull, transferring the balance backwards. This completes one full stroke.

(9) *Rhythm.*—Rhythmical action is essential in picking, and the proper timing of the stroke so as to bring the weight of the shoulders and the pick-head into play simultaneously as the earth is struck is a very important factor.

(10) *Other Factors in General Methods.*—(*a*) The pick-head should strike only solid earth, and not earth already loosened; thus a systematic method of picking from left to right is essential, and the earth should be gradually brought down to the feet in layers.

(*b*) The use of the needle point in hard stony soils and of the chisel point in softer and clay soils is a point to be insisted on.

Subsection III.—CYCLEGRAPH STUDY OF DIGGING.

To determine the actual motion paths of the pick and shovel when in use and the body movements employed in their actions, and thus to study in the skilled and unskilled workers the component parts of one complete pick action or one complete shovel throw, a method has been devised which gives a photographic record of the continuous path of a cycle of motions.

The method consists of fastening tiny electric light bulbs to any part of the subject or tool whose motion path it is desired to study. A photograph is then made of the moving part to which the light is attached during the time that this part is performing the operation. By using an interrupted current at a known speed, the path of light can be made to consist of dashes with arrow-point ends, the arrow point showing the direction of the motion. This gives direction, relative time and relative speed. By having a penetrating wire-screen of known dimensions in front of the subject, the extent of the movements can be measured, and by a combination of all these factors, the motion cycle can be divided up and studied, and by taking records of various subjects an improved method can be devised by eliminating unnecessary movements and improving the necessary ones.

In our experiments the lights were attached in the study of shovelling to the T-piece of the shovel and to the forward hand. Also in some cases to the head and shoulder of the subject. Records thus gave the movements of the two hands during one complete cycle, and also the body movements which showed how balance was affected.

In the study of picking the lights were attached generally to the centre of the pick-head and to the forward hand; also to the advanced shoulder.

Conditions during the taking of these records were standardized as much as possible as regards soil, tools, and optimum rates.

The actual motion paths in picking and shovelling are shown and explained in the photographs attached (figs. 1, 2, and 3).

(1) *Shovelling.* (See photographs in figs. 1 and 2.)

Motion paths in one complete cycle show great variances in the skilled and unskilled but chiefly in one particular action of the cycle. This is in the backward swing of the shovel (after it is loaded with earth) prior to the throw.

General Observations on the Cyclegraph Study of Picking and Shovelling.

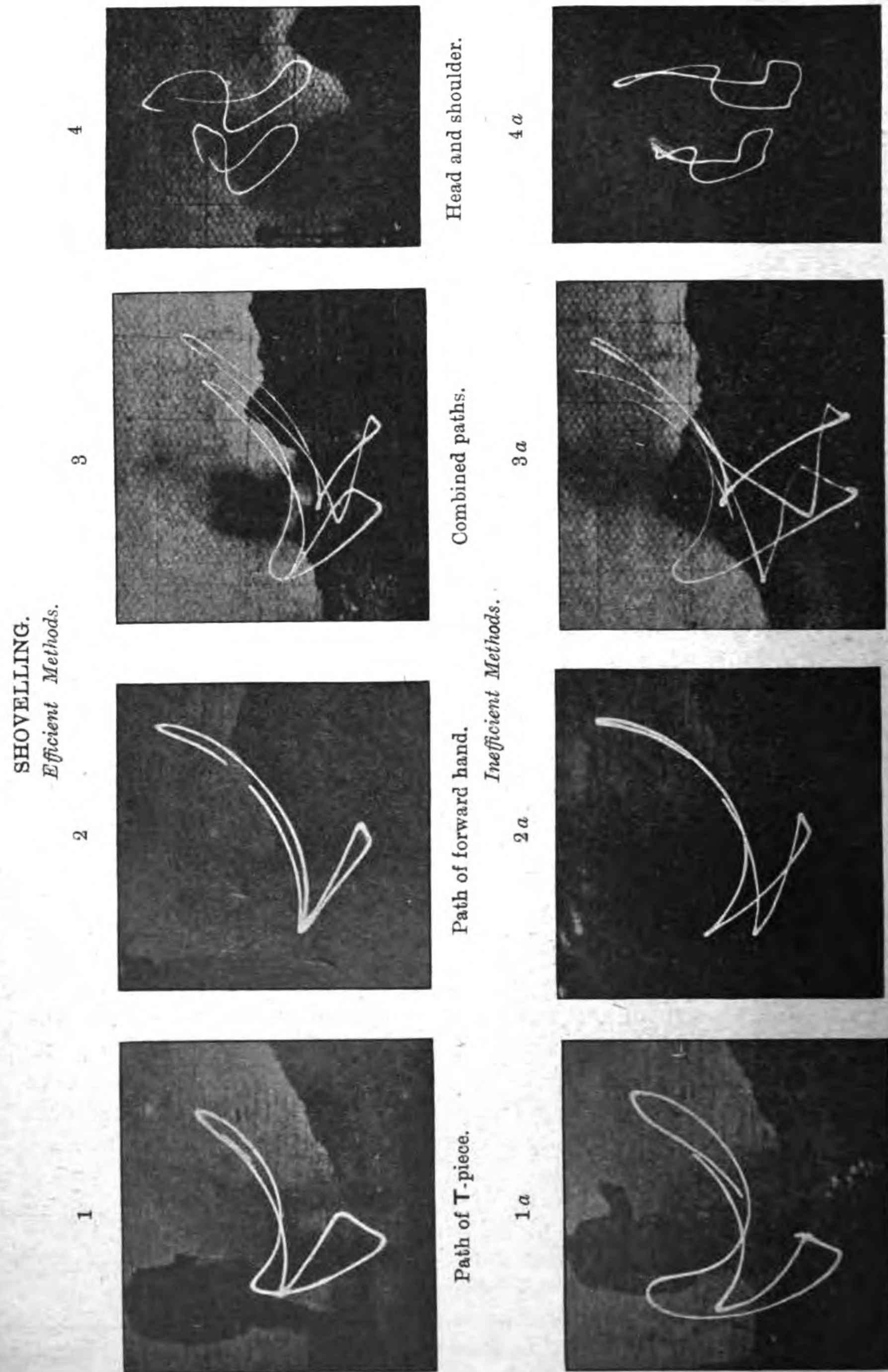
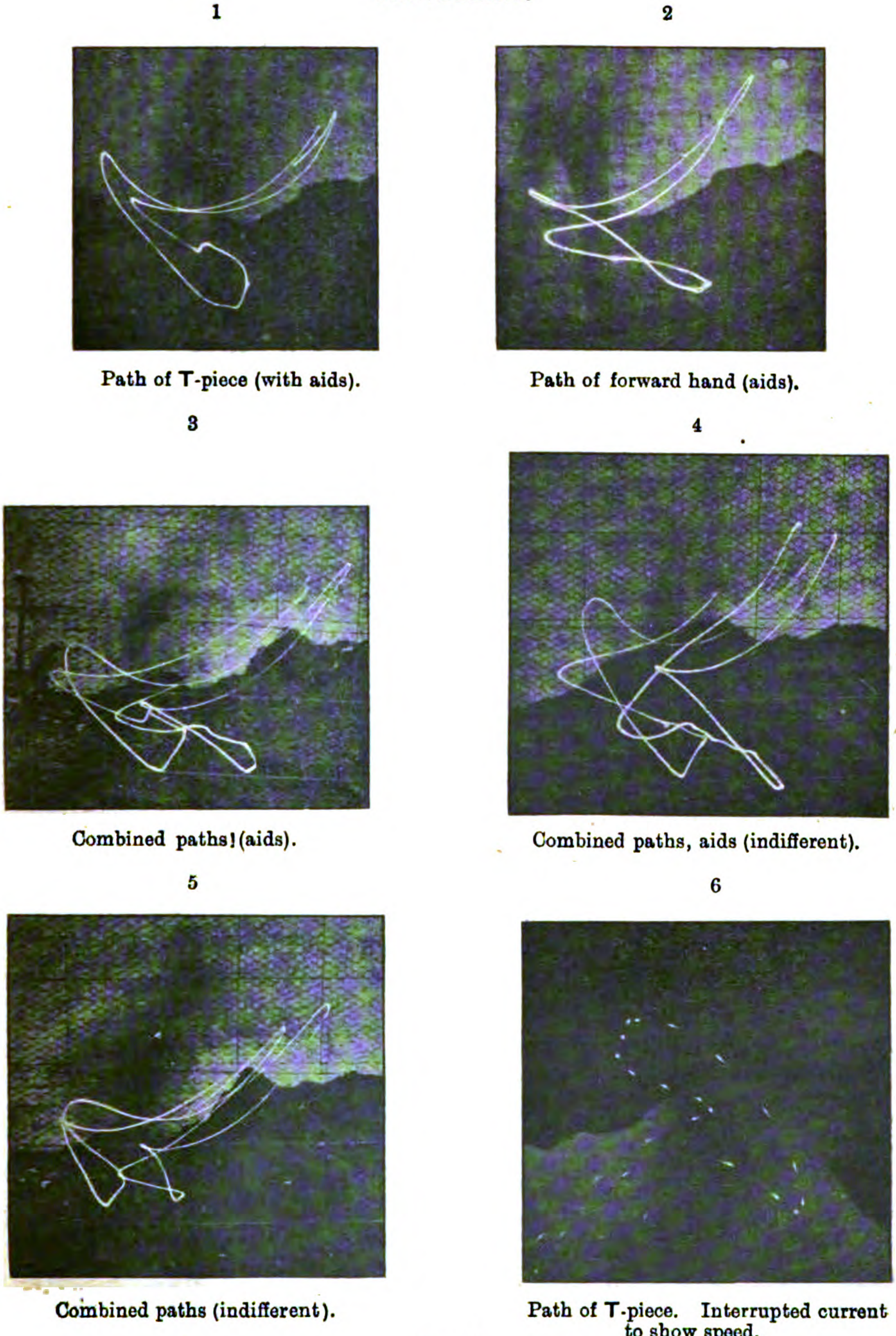


Fig. 1.

SHOVELLING.



1 Path of T-piece (with aids).

2 Path of forward hand (aids).

3 Combined paths (aids).

4 Combined paths, aids (indifferent).

5 Combined paths (indifferent).

6 Path of T-piece. Interrupted current to show speed.

FIG. 2.

In the skilled performer the path is low and follows back on the path taken by the shovel during the forward thrust into the earth in a pendulum manner.

In the unskilled performer this path leaves that of the forward thrust and rises to an appreciable degree (generally the more inefficient the performer the higher the rise). This rise or lifting is caused by a straightening of the back and knees and means that for the final throw the actual casting away of the load must be done chiefly by the muscles of the arms and shoulder. That is, in the unskilled subject the casting away of the load is performed in two stages:—

(1) Knees and back straightened during the backward swing of the arms.

(2) Load cast away by an arm swing.

It will be seen that this is not economic and must tend both to destroy rhythm and balance and to increase energy cost.

On the other hand in the skilled performer the casting away of the load is done as follows:—

(1) Backward swing of the shovel with the arms at full stretch keeping the knees and body bent.

(2) Load cast away by a combined straightening of the knees and trunk and an arm swing.

Here we have muscle forces coming into play simultaneously thus reducing energy cost and favouring rhythm of movement and balance.

Shovelling "with aids" shows an identical motion path to that without aids except in the forward thrust into the soil. This path becomes irregular and jerky where the additional body forces are being introduced (photographs fig. 2).

(2) *Picking.* (See photographs fig. 3.)

Motion paths of one complete cycle again show great variances in the skilled and unskilled, chiefly in the extent of the upward swing.

The unskilled performer invariably overswings, letting the pick-head in the upward swing fall well behind the shoulder. The weight of the pick-head has thus got to be raised to the perpendicular again during the downward stroke, thus entailing an extra energy cost. Again the weight of the shoulders is not utilized to reinforce the downward stroke. As the balance of the body and the shoulders must naturally come forward during this movement, output would be increased if this force were put to its proper use. The weight of the pick-head and the arms swing should combine to force the point of the pick into the soil.

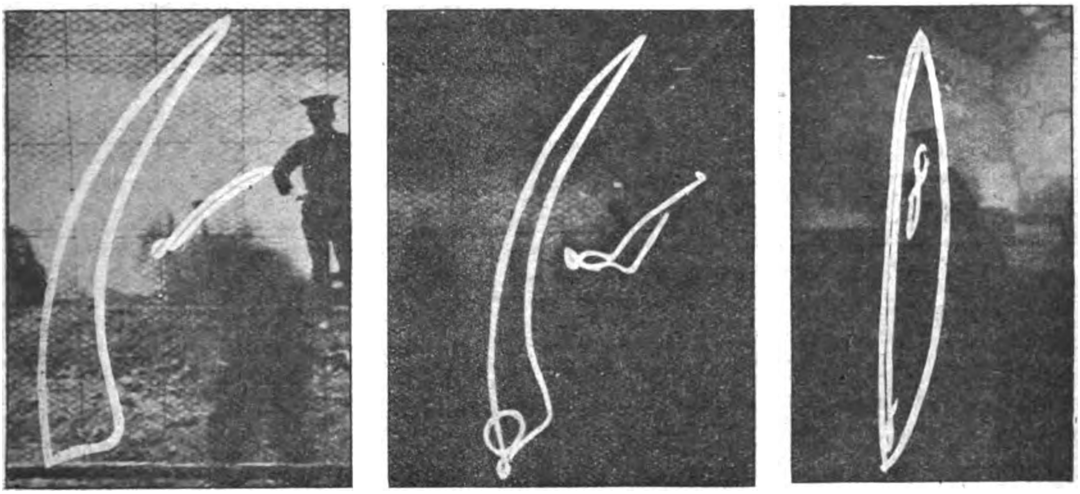
The skilled performer showed an upward motion path of lesser extent and the centre of the pick-head was never allowed to fall behind the centre of the shoulder, thus producing a more up-and-down swing. The shoulders come well forward during the actual striking movement and utilize the weight of the body to increase the force of the actual blow. Energy is thus conserved and output increased. The path of the forward hand shows

that this hand begins to slide down the helve just after the downward stroke is commenced.

The extra raking stroke employed by the navvies results in a circular path between the point of impact with the soil and the beginning of the upward swing and could possibly be combined with the break which would do away with this extra movement.

PICKING.—*Efficient Methods.*

1 2 3



Pick head and shoulder. Front-view.

1 *a* *Inefficient Methods.* 2 *a*

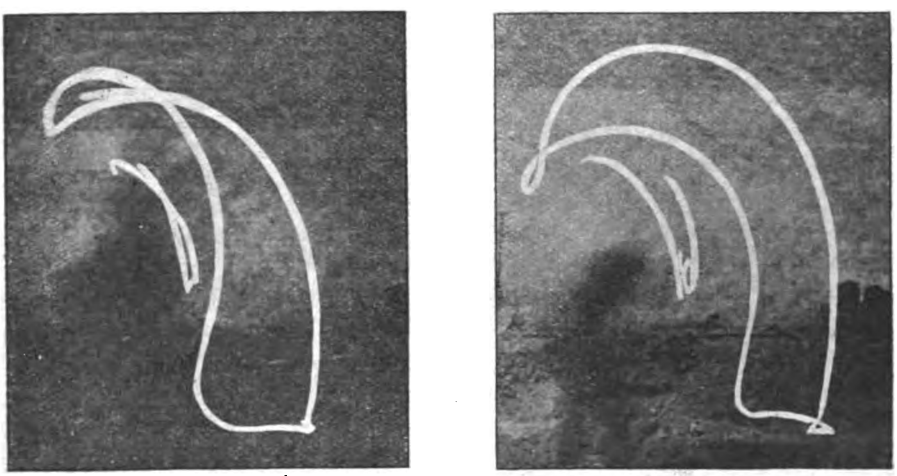
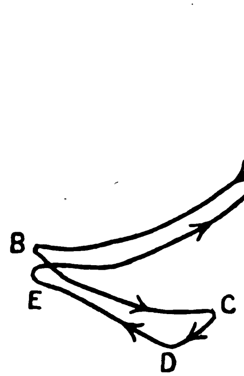


FIG. 3.

EXPLANATORY TABLE OF CYCLEGRAPH RECORDS IN FIGS. 1, 2, and 3.

Fig. 1.—Shovelling.

Efficient.—(1) Motion path of right hand (on T-piece) during one complete cycle; (2) Motion path of left hand (on helve near blade) during one complete cycle; (3) Motion path of both hands (as above) during one complete cycle.; (4) Motion path of head (in front) and advanced shoulder behind.



KEY ⟶

(Showing balance.)
A—B = Backward swing.
B—C = Forward thrust.
C—D = Depression of T-piece.
D—E = Backward swing prior to cast.
E—F = Cast.

Inefficient.—(1a) Motion paths of above. Shows too high rise of path DE caused by straightening the knees and body; (2a) Motion paths of above. Shows too high rise of path DE caused by straightening the knees and body; (3a) Motion paths of above. Shows too high rise of path DE caused by straightening the knees and body; (4a) Motion paths of above. Shows jerky and irregular paths, bad balance.

Fig. 2.—Shovelling.

Efficient.—(1) Motion paths of above. Showing the irregularity of path BC caused by reinforcing the thrust by pushing with both knees; (2) Motion paths of above. Showing the irregularity of path BC caused by reinforcing the thrust by pushing with both knees; (3) Motion paths of above. Showing the irregularity of path BC caused by reinforcing the thrust by pushing with both knees.

Inefficient.—(4) Both hands of above. Showing use of aids—Too high rise of path; (5) Both hands of above. Showing use of aids—Too high rise of DE; (6) Right hand. Interrupted current to show relative speed of paths.

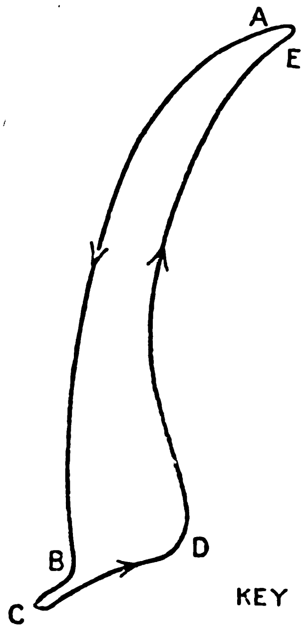


Fig. 3.—Picking.

Efficient.

(1) Motion paths of head of pick and advanced shoulder (behind).

(2) Motion paths of head of pick and advanced shoulder (behind).

(3) Motion paths of head with raking stroke (front view).

Inefficient.

(1a) Motion paths of above. Show dropping of pick head behind shoulder.

(2a) Motion paths of above (path DE), and too high rise of path AB at beginning of stroke.

KEY ⟶

A—B = Downward stroke.
B—C = Raising small end of helve to break the earth.
C—D = Raking back the broken earth.
D—E = Raising head of pick upwards over shoulder for the next stroke.

(*To be continued.*)

ARMY HYGIENE ADVISORY COMMITTEE REPORT No. 3.

On the Maximum Load to be Carried by the Soldier.

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I.—Introduction.

(1) "It must therefore be the aim of every Commander to ensure that his Infantry reach their goal in the best possible condition for engaging in the hand to hand combat."—F.S. Regs. II.

(2) "The power of undertaking long and rapid marches without loss of numbers and energy is essential to success in war."—Inf. Training I.

War, quite properly perhaps, has always been thought of in the light of victory or defeat, of the triumph of strategy and tactics. In the public eye it is the ultimate end which has loomed largest and it will always be so. Even amongst those who do know, the actual engagements and the end bulk so very large that the fact is apt to be minimized, that the pawns in the game are men who must be fed, who are killed and must be replaced, if the war is to be waged successfully. Obviously it is no more possible to conduct a war without casualties than to make an omelette without breaking eggs, yet the wastage of war is just as great behind the lines as in the front trenches. An army to be fit for its job must be recruited from healthy men of good physique but these men must be kept fit. The human organism is not capable of enduring a limitless strain. The soldier must be well fed: he is. The soldier must be well clothed: he is. The soldier must be well trained: he is. The soldier

must transport a reasonable amount of equipment and personal belongings, *but* there is a limit to his performing the dual rôle of beast of burden and fighting soldier.

It certainly falls within the province of those entrusted with the actual conduct of a campaign to direct the army to attack this or that objective, but if in the course of the campaign, as in the late war, due to the inclemency of the weather, the nature of the terrain or the evolution of new modes of warfare additional equipment is demanded and issued, they have no physiological right to demand that, as the load of the man is steadily added to, the soldier will remain effective as a fighting unit. The fighting value of a soldier is in inverse proportion to the load he carries.

The problem is infinitely more far reaching than the mere fatal diminution of the fighting value of the soldier. As the load the man is made to carry increases, the cost in energy for transportation increases. This energy comes from food alone, therefore the Q.M.G. is called on for more supplies. Not only so but other branches of the supply department are taxed as with the increase of the load the desire, and even the need of the man to shed part when and where he can, increases. (Vide the roads in France in the War.) Nor must it be forgotten that the over-fatigued man, especially if it be fatigue induced by the carriage of unduly heavy equipment, is usually a man of low morale. Yet "success in war depends more on moral than on physical qualities"—F.S. Regs. II. The resources of the A.G. are also severely taxed. The man power of the country is limited, the division between men required for fighting and for manufacture of essential munitions is nicely balanced. The overloaded man can easily become a physical wreck before he is even near the front line. Another pensioner is added to the country's post-war burden and the A.G. has to find a new man for the line.

The two following quotations express the same idea. Thus in the Report of the Committee "appointed to inquire into the effect on health of the present system of carrying the accoutrements, ammunition and kit of Infantry Soldiers," Report II, 1867, it is stated, "We need not discuss a matter notorious to all the world, but may state without fear of dispute that the conditions of modern war demand that the marching powers and endurance of the soldier must not be lessened by unnecessary weight or by a defective mode of carrying the weight. *Cæteris paribus* the army that is least weighted and that can move with the greatest rapidity must have the advantage." Also "we are of opinion that his (the soldier's) efficiency and health suffer far more from his carrying too many things than from his occasional and temporary absence from any two or three articles of his kit."

Parkes, one of the greatest of all authorities on Army Hygiene, who was a member of the above War Office Committee, also expressed himself in his textbook as follows: "In time of war it is most important to have

the soldier as little weighted as possible. The long and rapid marches which have so often decided wars have never been made by heavily laden men. The health also suffers. It is of national importance that the soldier should be as healthy and efficient as possible, as the fate of a nation may be staked on the prowess of its army. The line which the weight of his necessaries should not exceed should be drawn with the utmost care; if his health suffers more by carrying some extra pounds of weight than it benefits by the comfort the articles give, why load him to his certain loss? The overdoing of necessaries of the soldier has always been a fault in our army. Robert Jackson, cited by Parkes, noticed it seventy years ago. 'It is a mistake,' he says, 'to multiply the equipment of the soldier with a view of adding to his comfort.'"

It is very obvious then that the determination, if it be possible, of the maximum economic load, i.e., the maximum load which may be transported by a man who can yet after a march remain an effective soldier, is most desirable. On this capacity of the man to remain effective will pivot all possible plans for offensive action drawn up by the general staff.

II.—Load.

Lothian in his admirable historical report on "The load carried by the Soldier," has destroyed the mythical stories of tremendous marches of soldiers carrying extraordinary loads, such as that of the famous sixty pound load reputed to have been carried by the Roman legionary on his long marches. Parkes too had previously doubted some of these stories. He states in his textbook that the famous march of Craufurd's Light Division (43rd, 52nd and 95th Regiments of Foot) at the Battle of Talavera, of sixty-two miles in twenty-six hours with only seventeen stragglers, which both Napier and Moorzom state was carried out with the men carrying loads between fifty and sixty pounds, was done because the men carried only a shirt and a spare pair of either boots or soles. His authority was Lord Clyde who informed Professor Longmore (Army Medical Service), that he had seen the Division come in.[1]

It is the faulty account of such marches that has encouraged the belief that men can, if they are called upon, perform wonderful feats. These wonderful marches may be possible with carefully selected units, but they are not possible of accomplishment by the average units. It is largely a question of physique.

Lothian also dealt with the relation of the load to the physique of the soldier and pointed out the evil results which follow from overloading not only the healthy normal recruit, but, more particularly, the soldier who has been debilitated by disease and exposure. Excellent as was the hygiene, in the broadest sense of the word, in the late war, great as may

[1] Oman ("Hist. Penin. War," II, p. 560) says "forty-three miles in twenty-two hours with fifty pound load."

be the advances which will yet be made, hygiene can never be so perfect—the exigencies of war will certainly not permit it—as to eliminate completely the effects of exposure and the ordinary normal debilitation which must result when recently incorporated and trained men are put in the field. The problem of the exhausted man will never be solved even in the most perfect army put into the field for a prolonged campaign. The harder the fight the greater the resulting exhaustion. We must count then in the assessment of the maximum economic load on a gradual deterioration of the physique.

The load too must be correlated with the weight of the soldier, indeed it may be regarded as a function of the weight. Lothian has given a good account of the loads which have been carried by soldiers since historical records were available, and how this load has fluctuated with the changes in the art of war. His graph (fig. 18 in the Report) demonstrates in a most vivid fashion the gradual but steady increase of weight which has taken place in army equipment. His figures are no mere fanciful estimations; they are based on actual data.

The result of the various developments in offensive measures, in addition to the fact that the war in France, at least, was mainly stationary, led to the soldier being overloaded. Probably the average load of the infantry soldier in France was equal to about sixty per cent of his body weight, and it was not uncommon to find individuals carrying, or rather staggering under, a load which was equal to seventy-five per cent and even more of his body weight. In calculating the load of the soldier it is very frequently forgotten that a march on a wet day over muddy roads, or even a good heavy shower of rain will bring about a very material increase in the weight of the load borne. For instance, if the greatcoat gets well soaked, exclusive of adherent mud, this will mean an addition of about twenty pounds to the load and the addition of mud, together with the water in wet equipment, trousers, puttees and boots, adds at least an additional fourteen pounds. Thus if a soldier started out with an initial load of say sixty pounds on a march on a wet day through muddy country, he would finish with a load of ninety-four pounds or more.

This steady taxation of the soldier is not confined to Britain, as reference to Lothian's report, for example, will show.

The question as to whether there has been an increase in the physique of the soldier which would justify the steady increase in the load naturally arises. It can very definitely be stated that there is no such increase; indeed, with the increasing urban population and decreasing peasantry, there is actually a steady decline in physique. Pre-war, the average recruit of 19 years weighed about 127 pounds, whereas post-war the average weight, according to Sylvester Bradley's figures, is 122 pounds. Obviously, with the improved feeding and steady army training the recruit puts on weight, and we are inclined to agree that the figure put forward by Lothian for the average weight of the trained soldier, of 135 pounds is approximately

correct. If this figure be accepted as the average body weight of the British infantry soldier it is now essential to determine the maximum economic load in terms of the percentage of the body weight.

III.—Previous Work.

The great majority of the observations which have been made on the subject of forward progression (walking or marching) have been carried out with the virtually unloaded subject. These experiments, however, are of considerable, indeed of vital, importance, because after all, no matter if the subject be stripped naked, walking entails the forward movement of weight. The difference between the naked pedestrian and the loaded soldier is only a matter of the number of kilos to be transported, although of course in the one instance it is more or less evenly distributed live weight and in the other an external load is added, which may or may not alter the centre of gravity of the body, and hence the position of the load is important. A load which could be carried with readiness if properly placed on the back of the subject would obviously so handicap as to render him useless if it were attached, let us say, mid-way between the knees and the hips.

A very full discussion of the earlier literature, on the energy transformation during horizontal walking is given in Benedict and Murchhauser's (1915) monograph. Using the horizontal kilogrammetre as the unit it is found, as Durig (1911) had previously maintained, that, if the normal rate be held to be from 80 to 90 metres per minute, the gramme calorie value per kilogrammetre lies between 0·3 and 0·7 with a very distinct tendency to approach a mean value of 0·55 gramme calorie. The experiments, on which this mean figure is based, include marches done with and without loads.

The most important of these investigations for the present report will now be briefly discussed. Zuntz and Schumburg (1901) carried out a long series of observations on the energy expenditure involved in military marching.

They came to the conclusion that, speaking generally, the energy cost in marching increased almost proportionally to the mass moved, but, that under favourable conditions (the *position* of the load being of prime importance) the superimposed load cost less for forward progression than that of the body in an unloaded condition. They found, however, in certain experiments, that the amount of the superimposed load did materially influence the cost.

The experiments also of Brezina and his associates are of primary importance as the question of the influence of load is fully discussed. Brezina and Kolmer (1912) confirmed the finding of Durig, viz.: that the maximal economic velocity is approximately eighty to eighty-five metres per minute. They also found that the cost was not influenced by loads up

to twenty-one kilograms, i.e., this amount of extra dead weight could be carried as economically as so much extra live weight. As their subject weighed seventy-one kilograms this means that, under their conditions, loads equal to approximately thirty per cent of the body weight might be regarded as being equivalent to so much body weight. Heavier loads they found brought about both an absolute and relative increase in the energy output. They also maintained that it was more economical, as regards energy output, to increase the load than the speed at which it was carried, i.e., the maximal economic velocity fell with loads increasing above thirty per cent of the body weight. Brezina and Reichel (1914) re-examined these results in an endeavour to obtain a mathematical statement which would allow of generalization. They came to the conclusion that: (1) for moderate rates of progression the cost per horizontal kilogrammetre was independent of the speed and is smallest at 0·5 calorie for loads of approximately nineteen kilograms, i.e., about twenty-seven per cent of the body weight, and (2) the energy increase for loads exceeding nineteen kilograms was proportional to the square of the load difference. When the maximal economic velocity was exceeded they held that the metabolic cost increased in geometrical ratio to the arithmetical increase.

COST IN GRAMME CALORIES PER HORIZONTAL KILOGRAMMETRE.

Load in kilos.	March rate metres per minute 44·7—49·7	68·9—73·3	89·9—92·0	111·4—118·1	141·0
3	0·48	0·60	0·57	0·77	0·93
14	0·48	0·47	0·62	0·93	—
24	0·57	0·52	0·59	0·91	—
36	0·59	0·53	0·64	0·91	—
46	0·58	0·56	0·81	—	—
56	0·59	0·59	0·77	—	—

Benedict and Murchhauser (1915) published an exhaustive study of two subjects walking at different rates both before and after food. They found that in ninety-one experimental periods after food, with an average velocity during the period of 68·2 metres per minute, the heat output per horizontal kilogrammetre was 0·486 gramme calorie. The cost when the average speed was 111·4 metres per minute rose to 0·606 gramme calorie and at 146·3 metres per minute reached 0·907 gramme calorie, a figure greater than that when the subject ran instead of walked.

One of the very interesting determinations in this research was the measurement of the energy required for raising the body vertically during the act of forward progression, the elevation due to step movement. They found that one of their subjects, weighing seventy-three kilos, when walking with a velocity of seventy-six metres per minute expended 2·81 calories per minute above his standing basal metabolism, of which 0·65 calorie was expended in raising the body through a distance of approximately four metres per minute. This means that about twenty-three per cent of the total expenditure was required for elevating the body.

Cathcart and Orr (1919) in their study of the energy expenditure of the infantry recruit in training studied the cost of marching with varying loads and velocity under field and laboratory conditions. They used in their field experiments eight different subjects who carried three different weights, 15·3 kilos (drill order), 20·5 kilos (fighting order), or 25·0 kilos (marching order). The maximum load 25·0 kilos varied as a percentage of the body weight of the subjects tested from 32·0 per cent to 54·3 per cent, and for the average 135 pounds man would have formed about 41 per cent.

In the determination of the cost of the load the rate of marching selected was 100 yards per minute on a good level road. It was found that the average cost in gramme calories per horizontal kilogrammetre for the three loads was 0·543 for the lightest, 0·638 for the medium, and 0·672 for the heaviest load.

The second series of observations were carried out in a laboratory on a single subject weighing 62·6 kilos. The loads carried were 11, 16, 21 and 26 kilos, forming respectively 17·5, 25·5, 33·5 and 41·5 per cent of the body weight. In addition to varying the load the influence of variation in the velocity of marching was also investigated. The results are summarized in the following table:—

Velocity in metres per hour		Load in kilos. 11			16			21			26	
		A	B		A	B		A	B		A	B
3428	..	0·52	44	..	0·45	41	..	0·49	47	..	0·48	49
4368	..	0·56	41	..	0·51	40	..	0·57	48	..	0·55	49
5486	..	0·60	44	..	0·61	47	..	0·60	50	..	0·63	55
6039	..	0·66	48	..	0·65	51	..	0·65	54	..	0·70	62
6583	..	0·69	51	..	0·71	55	..	0·71	59	..	0·72	63
8778	..	—	—	..	0·84	67	..	0·89	75	..	—	—
10,972	..	0·85	63	..	0·81	64	..	0·83	69	..	—	—

A = cost in gramme calories per horizontal kilogrammetre.
B = ,, ,, ,, metre.

In a further short series of laboratory experiments with three subjects carrying loads of nine and twenty-six kilos, the lighter load being about fourteen per cent of the body weight, and the heavier about forty-one per cent at 55, 82 and 110 metres per minute, the cost in gramme calories per horizontal kilogrammetre for the nine kilo load was for the three speeds 0·48, 0·51 and 0·65, and for the twenty-six kilo load 0·53, 0·54 and 0·70 gramme calorie.

Benedict, Miles, Roth and Smith (1919) in the course of their valuable inquiry into the effects of a restricted diet on the cost of performance of work, compared the energy expenditure of a group of twelve young men under normal conditions of feeding with that of the same group after they had been on a restricted diet for twenty days. They found that the average cost in gramme calories per horizontal kilogrammetre for the men on the normal diet was 0·597, and after twenty days on a restricted diet it

was 0·592 gramme calorie. They also made a number of observations on a group of eleven men who had been kept on a restricted diet for 120 days and who were much below their normal weight, and found that the cost per horizontal kilogrammetre had fallen slightly, to 0·522 gramme calorie. The rate of marching, which was done on a treadmill in a closed chamber, was about seventy metres per minute.

Liljestrand and Stenström (1920) using the Douglas bag method, experimented on men walking on a good track. The influence of velocity of movement was alone tested. They found with their two subjects, who differed markedly in weight, the following figures:—

(I) The cost in gramme calories, and (II) oxygen intake in cubic centimetres per horizontal kilogrammetre.

Velocity per minute		I			II	
		A 80 kilos	B 60 kilos		A	B
50—75 metres	..	0·517	0·491	..	0·107	0·101
75—100 „	..	0·613	0·574	..	0·127	0·118
100 „	..	0·830	0·710	..	0·172	0·146

They suggested that the oxygen intake was a better guide than the heat output, as the calculated calorie value depends on the respiratory quotient which is readily altered by variations in the carbon dioxide output.

Cathcart, Lothian and Greenwood (1920) criticized the formula put forward by Brezina and Reichel on the grounds that the data on which it was founded did not justify the conclusions. They showed that the relation between the energy cost per unit of time and speed may be equally well represented by another type of formula. They did not believe that either the formula of Brezina and Reichel or their own was the expression of any physiological law. They applied their formula to a collection of experimental data, obtained for the most part from one trained subject marching at approximately 55, 82 and 110 metres per minute, and found that their optimum rate, about eighty-two metres per minute, agreed very closely with the maximal economic velocity of Durig.

Finally, Smith (1922) has just published from Benedict's laboratory a large monograph on metabolism during level and grade walking. He found for horizontal walking that the average value of eight men, at speeds mostly below eighty metres per minute, was 0·538 gramme calorie per horizontal kilogrammetre. He also considered the influence of velocity on the cost per horizontal kilogrammetre, and found, like many other observers, that with speeds below eighty metres per minute the influence of the velocity was not at all marked. It may be noted, however, that at the lowest speeds tested, thirty-five to forty-five metres per minute, there was a tendency, although Smith did not commit himself to this as a definite conclusion, for the cost to be a little higher than at faster rates. The suggestion, however, in view of Frentzel and Reach's (1901) statement that

very slow speeds are somewhat more costly than moderate speeds, a conclusion questioned however by Durig (1909), is decidedly interesting. Smith has also an excellent discussion of the percentage cost of the total energy expenditure in walking due to step lift. He found that step lift accounted for about nine per cent of the energy expended in horizontal walking at a rate of 43 to 48 metres per minute, about 11 per cent at 52 to 58 metres per minute, 15 per cent at 60 to 68 metres per minute, over 16 per cent at 71 to 73 metres per minute, and 18 per cent at 76 to 78 metres per minute. These results would be of prime importance if the question arose of altering the nature of the gait with a view to reducing the cost of marching. The "pas de Raoul" or "marche en flexion" is a case in point—see also Cathcart and Orr (1919).

(*To be continued.*)

HABITS, CUSTOMS AND MODES OF LIFE OF THE NATIVE TRIBES OF BRITISH EAST AFRICA (NOW KENYA COLONY).

By Captain R. L. STANLEY, M.B.E.

Introduction.

These articles have been written with a view to placing on record some of the customs and habits of life of native tribes inhabiting that part of Africa until recently known as the British East Africa Protectorate and now as the Colony and Protectorate of Kenya.

They do not profess to deal with all the tribes in this extensive territory, but those selected and partially described may be regarded as the more populous and important.

A good deal has been written of late regarding this territory and its possibilities as a "white man's country." The natives in this connexion have entered largely into consideration and many well inclined persons at home view with consternation the circulation of unfounded stories telling of forced labour and what is represented as tantamount to a return to slavery.

I would assure all such that if emancipation and civilization are ever to succeed and supplant the savage and heathen ways of these tribes, it can only be accomplished on lines of education in matters of industry and Christian teaching.

Assuredly the civilizing progress already made amongst quite a large number of natives of all tribes will extend itself, and if here and there the tearing away from idleness, debauchery and heathenism constitutes a hardship, well, for the good of humanity, let it be so.

The male native of East Africa in his wild state is as a rule naturally idle beyond words, but takes care that his womenfolk are not so. The hewers of wood, drawers of water and tillers of the soil are the women, who perform also all domestic services for their idle lords and masters.

It stands to common sense therefore that gentle pressure must in some way be exercised to induce the idle manhood to come out and labour for his own benefit, that of his kinsfolk and humanity in general. Indeed this is the foremost step to be taken if ever the native is to be saved from himself.

His mode of life and customs which are briefly and imperfectly described in these pages may convey some small idea of the task which settler, missionary, and administrator has imposed on himself in his endeavours to spread the benefits of industry, religion and good government among such primitive peoples.

The illustrations will be more convincing, perhaps, than the letterpress in expressing the difficulties of the uphill task of those now engaged in

bearing the torch of light and the banner of freedom to these African people.

All who are anxious for the welfare of these wild aboriginal people, having read supposed or imaginary tales of hardship or stories of forced labour, may rest assured that their British kinsfolk in these parts are more than solicitous for the welfare of the native as regards his education in all useful ways and in persuading him to abandon his life of sloth and paganism.

Undoubtedly these apostles of industry, light and freedom will succeed in time, but probably centuries of work lie ahead before the whole vast population of East Africa will finally be brought under the wholesome influences now begun amongst them. In this work many pioneers have already gone under, staggered by difficulties and broken in health, whilst numbers have been doomed to rest their bones in their chosen fields of labour.

Let those who glory in the British Empire and this latest addition to it, the Kenya Colony, remember with pride the labours and trials of the noble men and women who have set themselves the uphill task of building homes amid savagery in tropical lands, and there to spread, by example and precept, the undying characteristics of the British race for the edification of peoples now in darkness, but who it is hoped may one day become an intelligent and solid bulwark of empire.

To the many officials, settlers, missionaries and others who have kindly furnished nearly all the information set forth in these articles, the compiler desires to express his grateful acknowledgment.

Nairobi, June 27, 1921.

Kenya Colony and its Peoples.

It would perhaps be safe to assert that no country possesses such a variety of inhabitants as Kenya Colony where every degree of evolution and civilization and practically every specimen of human society is to be found.

In the animal kingdom also the country is characterized by the number and variety of its fauna, and has earned for itself the name of "The hunter's paradise."

To go even moderately into details relative to the subjects referred to in the two preceding paragraphs would transgress the reasonable limits of this article.

There are extensive areas in the country with large human aggregations still in an uncivilized state, and here we can observe and appreciate the victory of even primitive man over hostile nature and compare barbarism with the relative advantages of civilization. As time goes on the distinctive characteristics of the primitive races of East Africa will no doubt disappear, and only partially described details, vague memories, or occa-

sional survivals remain to bear evidence of the conditions and environment of the natives of Kenya in the beginning of the twentieth century.

It is not always easy to obtain from the native reliable details in matters pertaining to tribal sociology, religion or its equivalent manifestations, sacrifices, spirits, ideas of future existence, etc., because of his decided reluctance to communicate or explain himself in these matters. The peculiarities described have therefore not been collected in every case with the ease of an ordinary investigation. The practices and customs of these people to-day can safely be regarded as those of their ancient organizations and probably in no way conflict with ideas entertained thousands of years ago.

For geographical classification the country may be divided into four sections :—

The Coast belt.	The Lake area.
The Highlands.	The Desert area.

Each section possesses its own geographical, geological and climatic peculiarities. The chief characteristic of a great area of all zones, excepting the desert, is the great natural fertility of the soil. Agriculture and the raising of flocks and herds easily take foremost place in the economic life of all tribes, the settled pastoral life prevailing except in arid regions where periodic migrations have to be made for grazing purposes.

The country is traversed in a westerly direction by the Uganda Railway running from Mombasa to Kisumu, on the Victoria Nyanza, and in its zig-zag course covers a track of nearly 600 miles. On the journey from the Indian Ocean to the great inland lake every variety of climate and temperature is experienced. At its termini, Mombasa and Kisumu, tropical temperatures prevail, whilst between Limoru and Lumbwa, cold and even frosty nights compel the traveller or settler to seek the warmth of blankets. The great snow-clad mountains of Kilimanjaro and Kenya are visible from points of vantage on the journey. Almost every conceivable variety of game is to be seen on the plains which are in reality a vast natural zoo of extraordinary interest and impressiveness. The great Rift Valley, which cleaves the African continent and conveys to the mind some idea of the colossal magnitude of the volcanic upheaval that caused such laceration, is traversed by the railway on a section of its escarpment. At Mau the train has reached an altitude of nearly 9,000 feet above sea level. There is from thence onwards a gradual drop until the great lake is reached which is 3,000 feet above the sea level.

Most of the various tribes referred to in the succeeding chapters will be met or seen on the rail journey. The Swahili at the Coast dressed in a variety of costume and colour, the natives of the interior clad in skins, and the Kavirondo living in the low country round the lake who, perhaps, alone amongst peoples view with contempt any effort at clothing their nakedness.

The population of the country has been estimated lately as near 4,000,000

native inhabitants, but no accurate census has so far ever been taken. There is a European population of about 10,000 with some 30,000 Asiatics, engaged chiefly in agricultural, industrial and commercial pursuits.

The main centres of non-native population and industry are Mombasa, Nairobi, Nakuru and Kisumu, and all four towns are on the railway. The European agricultural settler is chiefly to be met with in the highland regions, but many have established themselves in plantations on the coast and in the Kavirondo country near the lake region.

The Wa-Swahili.

The Swahili may be said to be the descendants of successive waves of immigrants from Arabia and Persia, who arrived on the coast in dhows without their women and inter-married with the Bantu tribes they found there.

The Swahilis are chiefly resident on the mainland, at Zanzibar, and on the island of Pemba. On the mainland there are still separate tribes with chiefs and headmen of their own, but many different inland tribes having been brought up as slaves at the coast and on the islands near by, their customs have blended with those of the Swahilis. Slavery abolished many distinctions, such as chiefs, headmen, folklore and some distinctive customs.

The people live in villages and huts mostly of the rectangular shape, and constructed of sticks and mud, the inside walls, and sometimes the outside, being smooth and clean. The roofs are covered with thatch made from either grass or the leaves of the coco-nut palm. Between the walls and roof there is ample space for ventilation. Light is admitted by the door which is usually about five feet high: only in exceptional cases are windows provided.

Houses are built of rubble and stone also.

Villages are irregularly laid out and regardless of plan. Scattered groups of huts are usually to be found near cultivated areas. Each family occupies a separate hut, but where a man has two wives, the second is generally provided with a separate dwelling.

The coastal and island Swahili has been in touch with civilized people for some centuries, more particularly with the natives of India by whom he has been to some extent influenced and with whom he freely mixes in the towns.

The staple foods are maize, rice and wheat. Fruit and fish also are appreciated but comparatively little meat is eaten. Families eat together from a common dish, but when strangers come in for meals the women and children eat apart. Very little food is stored, and what may be is either hung up in the house in bags or buried.

Amongst the less enlightened Swahili poultry and livestock are admitted into the houses.

Tembo, an intoxicant made from the sap of the coco-nut palm, and some other native drinks are partaken of, but there is little drunkenness.

Medicine men are not numerous but have much local influence; happily their power is declining. Their methods are kept secret, and although they know of some useful herbs, on the whole their practices are bad and superstition largely aids them.

Iron filings, dross or chips from wrought iron, form a domestic medicine given for dropsy. The root of the mpukusu tree is mixed and cooked with coco-nut oil and used as a specific in skin diseases. A good deal of "cupping" is still done and, as a rule, with good results.

FIG. 1.—Swahili Men and Women.

Apart from Mohammedanism no theological system prevails or is taught and but few Swahili have embraced the Christian faith. The conversion of a Mohammedan to Christianity is a very rare event, as under Mohammedan law, which obtains at the Coast, he is automatically disinherited and divorced by his secession from Islam.

The men and women in some cases live to an advanced age—60 to 70 years. After death all classes of the community are buried either according to the Christian or Mohammedan law.

Cultivation of the soil is the chief occupation but many intelligent Swahilis are skilled builders, carpenters, railway workers and craftsmen, equal to, if not in some cases exceeding in ability the average Indian fundi.

The language spoken by this tribe will practically aid a traveller in his intercourse with others from east to west of Africa, as owing to penetration

in former and later times in the interior and across the continent there will always be found a native who understands the Ki-Swahili language.

Satisfactory figures of vital statistics are not obtainable. Small families of three to five children are fairly common and arrive at adult life. One must, therefore, come to the conclusion that the birth-rate is a fair average and the mortality rate relatively low. Females are in excess of males. Anti-conception and abortion methods are known and largely practised. Women rarely die in childbirth, labour is comparatively easy and the mother is usually about again three or four days after the birth. The child is nursed for about a year and artificial feeding is rarely resorted to.

Hinterland Tribes.

Many tribes are to be found in the hinterland, the principal being the Wa-Nyika and the Wa-Pokomo. The Giriama, Chonyi, Jubana, Kaumea, Kambe, Rabai, Ribe, Duruma, and Digo are all embraced in the generic term "Wa-Nyika."

The customs and habits of these tribes are very similar. They live in huts built of grass on a wooden framework and in detached groups; the number of huts in each group may be from four to thirty. In many cases the group is surrounded by a thorn fence or zariba. The huts vary in size, but are similar in design, the doorways being small, and the only means of lighting or ventilation. Husband and wife, or wives, with the younger children, may or may not live in one hut. It is customary as a matter of doing honour to a wife who has become the mother of a family to build a house for her and her children. Boys of about 10 or 12 years of age are sent to sleep with their fellows or in their grandmother's house. The latter must not occupy the same house as her grown-up son or daughter-in-law.

Villages are not laid out according to plan, and there are no sanitary arrangements, the bush being used for every purpose.

The food consists of all kinds of meat, including the flesh of leopards and other carnivorous beasts, elephants, rhinoceros, etc. Snakes are sometimes eaten, and also rats. The only creatures not eaten appear to be the hyena and the crocodile—although the latter is used as food by the Pokomo, who also include monkeys in their fare. Tribes and villages may, however, have their own forbidden animals. The vegetable part of the diet includes cereals and all the ordinary tropical fruits. The women and children eat together apart from the men, who eat with the headmen of the village. All eat in the open, but within the zariba and from a common dish. Palm wine or beer is the chief intoxicant, being taken mostly by the men, and not at meals. A good deal of drunkenness goes on among the elder men of the tribes.

In each hut there is a fireplace for cooking purposes. Some five or six feet above the fireplace is a framework or basket of sticks which is used for storing food. Maize and rice are kept in bags made from palm leaves.

Goats, sheep, and fowls are kept in the dwellings, and cattle have a pen in the village.

Dependants other than a man's own children are inherited property and are maintained by the inheritor and regarded as his relations.

A person suffering from smallpox or other markedly infectious disease is removed to a hut in the bush, and his house with contents burned. Food

FIG. 2.—Hinterland Types.

is taken from the village and placed in the neighbourhood of the hut. This procedure is followed until the patient either recovers or dies. If death follows, his family bury the body and burn the hut.

Domestic medicines are made from herbs, roots, and leaves for various ailments treated by the medicine men. Besides the use of herbs they practise cupping, burning, and scarification. It is not so much that medicine is practised by any set class of men, as that many individuals

possess the knowledge of one or more medicines (which knowledge is purchased) and use their skill on the diseases which they profess to understand. Thus one man will treat one kind of illness, and another will be resorted to in a different ailment.

At the same time there are certain men who are in a special sense "medicine men," who profess more or less supernatural powers, exorcise evil spirits, take off spells, and the like. These are by no means elderly men or persons of any special standing, but are often comparatively young men. They have no particular influence in tribal matters and are under the control of the elders just as other members of the community. They teach no theological system, but call upon the shades of past medicine men to aid them in their work. No system of theology is taught, but an animistic cult is chiefly concerned with the spirits of the dead, though God the Creator is addressed in prayer together with the shades. Offerings of food and drink and animal sacrifices are made at the graves of the dead, and it is believed, unless constantly propitiated, that the attitude of the dead towards the living is malevolent.

Most of these tribes do not believe in any after-life.

Deformed children are frequently destroyed at birth, as it is considered they will spoil the rains if allowed to grow up.

Little clothing is worn by either sex, a goat or sheep skin or a piece of calico (americani) which suffices to cover the loins when hung from the shoulder, being practically the only dress for all occasions.

Large families of from five to ten children are not uncommon, and arrive at adult life. The birth-rate would appear to be high and the mortality low.

The Wa-Taita.

This people inhabit the Taita district some 150 miles from the Coast, a mountainous, hot and arid region for the greater part. The railway passes through it at Voi.

Their dwellings consist of the usual grass and brushwood type, built in circular form, and any number may form a village. Each wife has her separate hut, and the husband lives with each of his wives in turn as fancy takes him. Girls live with their mothers till marriage, and boys till about puberty, when they live in huts apart, about four or five in a hut.

The plan of a hut of a married woman is laid out on lines as follows :—

The huts have no light or ventilation other than that provided by the door which is a few feet high and two and a half wide.

Villages are not laid out to any plan, but the endeavour is to find a sufficient flat area on the steep hill sides. No sanitary arrangements exist, and the bush provides the dumping ground for all filth and garbage. Goat and other animal dung is used for manuring the shambas (vegetable plots).

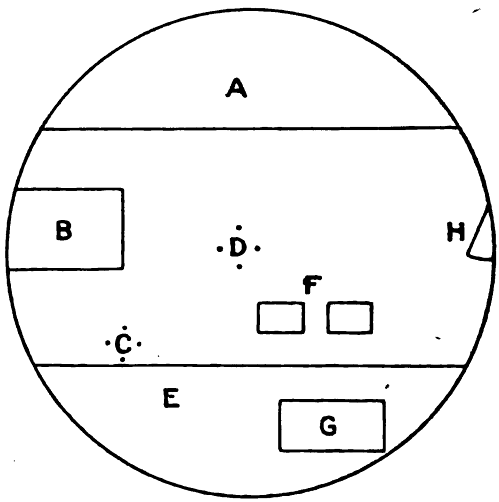
The Wa-Taita have a considerable variety of food in matama, mahindi maize, sweet potatoes, meat, game, fowls, etc. The food is cooked in

native-made earthenware pots, and all persons eat together, but out of separate dishes. The same dishes are provided for both sexes, old and young, but women are not allowed to eat fowls or the flesh of an animal that has died in giving birth, the latter for superstitious reasons in relation to child-birth. Liquors of an intoxicating nature are made from honey and sugar cane, and are consumed at all times and on all ceremonious occasions.

Diseases of an infective nature are common, but, as in all tribes, an elementary conception of segregation is entertained. This, however, is too carelessly carried out to be even moderately effective.

Native medicines and charms are numerous enough to provide the medicine men with varied formulæ for many ills. The fæces of a brown beetle of which a stock is kept alive are used for coughs. Powdered roots, barks and leaves, also decoctions made from these, are administered inter-

A. Space for goats and calves.
B. Bed.
C. Pots, Water.
D. Fire-place.
E. Poultry.
F. Children's beds.
G. Bed of young male.
H. Entrance.



nally or rubbed in externally as advised. A piece of leopard flesh mixed with powdered charcoal is chewed and spat in the direction of wild animals to make them go away. It is usual to consult a "diviner" before taking medicine.

The "diviners" in religious matters had at one time much the same power as chiefs. The form of faith in this respect appears to take two lines: (1) the worship or propitiation of departed relatives, and (2) the worship of idols. The worship of ancestors consists in offering sacrifices to the skulls of the loved ones gone before. As the departed are supposed to converse with the living through the medium of dreams, they are not regarded as having ceased to exist, and the skull is regarded as the one time container of the spirit. The skull of any person who has produced children is exhumed after a year and placed in a cave with other ancestral skulls.

A deceased father or mother, unless propitiated by sacrifice, is regarded

as the cause of illness or failure in an undertaking, and his skull is regarded as the depository of his spirit. A "diviner" is consulted as to which parent is the cause of the trouble, or is asked to give advice. If the skull of a parent has not been exhumed, the supplicant may be advised by the "diviner" to place it in the sanctum for skulls. A goat or ox is then sacrificed, and parts of its inside are smeared over the parental skull. Those taking part in the ceremony must eat the whole of the sacrificed carcase. No woman or child is allowed to take part in this sacrificial ceremony.

Charms or idols are worn or carried on the person for many purposes, and are believed to be effective. Black beans and a white shirt guard against ill health; a large seed or nut filled with medicine stops enemies coming over the border; a small stool prevents illness of man or beast; two small knives signify good crops and herds; an ostrich egg provides against loss of property; and the skin of a hedgehog guards against the death of a parent.

The ceremony of imparting to charms the necessary powers is one which is carried out by the elders and extends over a week, the dedication taking place on the seventh day, when a feast is given and the new charm smeared with the blood of the sacrifice, after which it is ready for use. Each married man with a family can petition his own charm or idol, but on special occasions various families of the same clan may bring their mafukos (charms) together and place them on an altar, when the service of an hereditary mganga (medicine man) is required. The elder who presides on the occasion throws meat to the mafukos and squirts beer from his mouth over them, after which there is a general feast.

The dead are buried close to the dwelling huts for superstitious reasons, and after an interval (four to twelve months) the skull is exhumed and placed in a grotto, when in the event of any trouble or sickness in the deceased's family, sacrifice is offered to the skull (repository of spirit) and the people go into mourning for seven days. A goat is slaughtered as a sacrifice, which the relatives of the deceased afterwards eat.

Albinos are frequently given insufficient food so that they may die; deformed children are not neglected, but on the contrary are given more attention than others.

The custom of circumcision is made an event of great celebration, feasting, dancing, shrieking and drinking being outstanding features. The effects of the merriment are apparent for many days.

There is no reliable data to estimate the vitality figures of the tribe, but the indications are that the women are reasonably fertile and infant mortality is heavy. The child is usually nursed for nine months. When artificially fed this takes the form of a concoction made from stewed bananas and cow's milk placed in a gourd and held to the lips. After birth the mother remains in her hut for six or seven days and then goes about her ordinary everyday work as usual.

(To be continued.)

Clinical and other Notes.

A CASE OF SARCOMA OF THE SPHENOID.

By Majors J. H. SPENCER and H. GALL.
Royal Army Medical Corps.

Drummer W., aged 33, admitted to Queen Alexandra Military Hospital on October 10, 1922, for pain in the right side of the face and spasm of the jaw.

History.—He had complained of pain in the right side of the face for eleven months previous to admission and had had two teeth extracted in May, 1922, without resulting benefit.

Between May and September in this year he was treated for trigeminal neuralgia at another hospital, with some alleviation of the pain in the upper part of the face but none in the lower.

On admission patient was pale and anæmic, depressed and neurotic. The examination of his nervous system disclosed the following condition:

Fig. 1.

(1) The left side of the face was normal. On the right side there was hyperæsthesia to all forms of cutaneous sensation in the area supplied by the third branch of the trigeminal nerve, i.e., over the lower jaw, lower lip, tragus of the ear, and anterior border of the external auditory meatus.

(2) The skin over the temple was affected to a slight extent.

(3) There was numbness of the right half of the tongue, which half was furred, the remainder being clean.

(4) Paresis of all muscles supplied by the motor branch of the trigeminal, giving rise to deviation of the jaw to the right on attempting to open the mouth.

The faradic responses (to such of these muscles as can be adequately tested) were diminished.

No jaw jerk could be obtained owing to trismus.

(5) Slight loss of tone in the lower right facial muscles, causing drooping of the angle of the mouth and some obliteration of the nasolabial fold. No change in voluntary or motional movements.

(6) Slight deafness in the right ear. This was reported to be catarrhal in origin by the ear specialist.

(7) Distinct atrophy of the right side of the tongue, which had a furrowed appearance and showed fibrillary tremors in the under part There was deviation to the right on protrusion of the tongue, but otherwise voluntary movements were unimpaired.

(8) No symptoms or signs of involvement of the other cranial nerves were found. The general nervous system appeared normal. Wassermann reaction negative; urine normal; ocular fundi normal; base of skull and right temporo-maxillary joint showed no abnormalities under X-ray examination.

Course of Case.—Early in November patient developed tonsillitis with an acute otitis media on the right side, which resulted in chronic middle ear suppuration. This attack was followed by loss of taste in the right side of the tongue and paralysis of the right half of the soft palate. The latter was very difficult to examine owing to the marked jaw trismus.

No evidence could be found of this paralysis being of diphtheritic origin.

During January, 1923, several attacks of epistaxis occurred and patient's general health became worse. There was however some improvement in certain of his symptoms, the deviation of jaw and trismus being considerably less than on admission.

Up to this time the opinion had been held that the case was one of true neuritis of the third division of the trigeminal nerve with involvement of the chorda tympani, and that this neuritis was of inflammatory origin. The occurrence of epistaxis placed a new complexion on the case, with the result that the opinion of Dr. Farquhar Buzzard, Consultant in Neurology to Queen Alexandra Military Hospital, was sought.

Dr. Farquhar Buzzard summarized the case as one of a chronic infective condition originating in the neighbourhood of the spheno-maxillary fossa, possibly tubercular or actinomycotic in nature. He recommended examination under anæsthesia by a surgeon. The case was examined under an anæsthetic by Lieutenant-Colonel J. West, R.A.M.C., and Major J. W. Weddell, R.A.M.C., on February 11, 1923, and the mouth forced open sufficiently to admit of examination. A large growth was felt which sprang from the basi-sphenoidal region and extended forwards and laterally. A portion of the tumour removed for section was on February 25, 1923, reported as a round-celled sarcoma by Lieutenant-Colonel H. Marrian Perry, R.A.M.C., Professor of Pathology, R.A.M. College. Patient is now undergoing treatment by radium at Middlesex Hospital. The writers are indebted to Staff-Serjt. Kearns, R.A.M.C., of the X-ray Department of the Queen Alexandra Military Hospital, for the photographs of the case.

A STAPLE HÆMOLYTIC SERUM FOR USE IN THE WASSERMANN REACTION IN TROPICAL CLIMATES.

COMMUNICATED BY LIEUTENANT-COLONEL H. M. J. PERRY, O.B.E.
Royal Army Medical Corps; Professor of Pathology R.A.M. College.

(1) COMPLAINTS have arisen from time to time from laboratories abroad regarding the rapid loss of titre of hæmolytic serum brought about by wide natural variations in temperature.

(2) In February of last year a report was made on the experimental trial of a dried (soloid) hæmolytic serum manufactured by the Wellcome Laboratories. This dried soloid serum had withstood prolonged high temperature with little diminution in value. Unfortunately, it has not been possible to maintain a supply of uniform quality owing to various difficulties in preparation.

(3) Further experiments have been made by Dr. O'Brien, Director of the Wellcome Physiological Laboratories, on the effect of adding glycerine to the ordinary hæmolytic serum, and this glycerinated serum has, under experimental conditions, remained unchanged for periods of three months at 37° C.

The following data bearing on this point have been supplied by Dr. O'Brien :—

EXPERIMENT 1.

Original titre of serum : 2400.

After incubation at 37° C. for three months—

Titre of glycerinated serum	...	2400
,, ordinary ,,	...	200

EXPERIMENT 2.

Original titre of serum : 3000.

After incubation at 37° C. for three months—

Titre of glycerinated serum	...	2300
,, ordinary ,,	...	375

EXPERIMENT 3.

Original titre of serum : 3700.

After incubation at 37° C. for three weeks—

Titre of glycerinated serum	...	3500
,, ordinary ,,	...	1000

(4) Samples of glycerinated hæmolytic serum have been tested at the Royal Army Medical College and found to be satisfactory. Further samples have been sent to China, India and Egypt to be re-tested after the next hot weather.

(5) It is suggested that when ordering hæmolytic serum for laboratories abroad the glycerinated product be requested.

Editorial.

THE ARMY DENTAL CORPS DINNER.

THE First Annual Dinner of The Army Dental Corps was held at the Trocadero Restaurant on Friday, May 11, with Lieutenant-Colonel J. P. Helliwell, C.B.E., in the Chair. Regular officers of The Army Dental Corps, temporary dental officers in the Army and the Royal Air Force, and all dental surgeons who served during the war as dental or medical officers were eligible to attend.

The total number present was eighty-two, including eight guests of the Corps.

It was noteworthy that distance was no deterrent to the attendance of many, one officer having travelled by air from Cologne, commencing his journey the same morning. Both officers and ex-officers from all parts of England and Wales, and also from as far north as Edinburgh were present.

The future success of this dinner as an annual institution seemed to be therefore assured. Dental officers are usually more or less isolated in "one man" centres and any occasion in which they can be gathered together to discuss their joys and sorrows will, if the first dinner is any criterion, always be well attended.

Mr. M. F. Hopson, late President of the British Dental Association, in proposing the toast of the Corps, made amusing references to pre-Corps days. He spoke of his appointment as teacher in dental surgery at Millbank, where, in a few lectures he was to impart to medical officers the knowledge necessary for dealing with dental defects of the troops. He was informed that he was entitled to the appellation of "Professor" but the appointment was something after the nature of a sinecure as no successor was appointed when he vacated the post. Mr. Hopson spoke of the efforts of the British Dental Association during the war towards the utilization of the dental surgeon in his professional capacity, efforts which culminated in the appointment by the Minister of National Service of the Dental Service Committee and eventually the Dental Tribunal. He spoke of the appreciation of Sir John Goodwin of the need for a dental service for the Army.

Major-General Pollock replied for the Corps. He mentioned that Guy's Hospital, of which Mr. Hopson was the first dental student, had provided more dental officers for The Army Dental Corps than any other hospital. He pointed out that the title of the Corps was *The* Army Dental Corps, the distinctive use of the article being an honour shared only by The Life Guards and certain other distinguished regiments. He was pleased that the dental service was put on a proper footing before there was any lopping by the Geddes Axe.

Lieutenant-Colonel Helliwell in proposing the "Visitors," read a

telegram from Sir John Goodwin expressing his regret and disappointment that unforeseen circumstances had prevented him from being with them that evening. Lieutenant-Colonel Helliwell regretted this unavoidable absence of Sir John Goodwin at their first annual dinner, especially as it was the last year of Sir John's term of office as Director-General. He would like in the name of the Corps to express to the Director-General their appreciation of his efforts in launching the Corps, which was one of the first public dental services. They considered him one of the pioneers in preventive medicine. They were pleased to have with them so distinguished an officer and scientist as Sir William Leishman. All knew of his work in preventive medicine, and as their branch was, they hoped, almost entirely preventive it came as a great satisfaction that he was, on the retirement of Sir John Goodwin, to be the Director-General of the Medical Services of which The Army Dental Corps formed part. They appreciated Sir William's presence and the fact that he had made a special journey from York to be with them. Colonel Helliwell also spoke of the practical interest General Pollock took in the well-being and efficiency of the Corps. He was very pleased to see with them the representatives of the four large dental schools in London. He particularly mentioned the assistance given by Mr. W. H. Dolamore in arranging for the acquisition of the bigger part of the Royal Dental Hospital for the treatment of troops during the war.

Sir William Leishman and Mr. W. H. Dolamore replied for the "Visitors." Sir William said that he was especially pleased to be present as his interest in dental science was no new one and he was looking forward greatly to a closer association with their Corps. He always found some difficulty in thinking of their respective professions as two and not as one, as in so many respects their work for the Army and their ideals were identical. He expressed his great appreciation of the work of the dental officers in the Commands which he had recently had the privilege of visiting with Sir John Goodwin. He referred to the importance of research in dentistry and trusted that this would be developed in the Corps and that a fruitful collaboration would be established with the research work in progress under the guidance of the Dental Board and the Medical Research Council. In this connexion he assured them that they would receive a cordial welcome in the Army Pathological Laboratories. Mr. Dolamore spoke in appreciative terms of the Corps and said that from what he had heard that evening he looked to the time when the whole dental profession would reach the same standard of efficiency as The Army Dental Corps.

The guests included Sir William Leishman, Major-General C. E. Pollock, Air Commodore D. Munro, Mr. W. H. Dolamore (Royal Dental Hospital), Mr. M. F. Hopson (Guy's Hospital), Mr. E. C. Sprawson (London Hospital), Mr. A. Bayford Underwood (University College), Mr. R. Lindsay (Dental Secretary of the British Dental Association), Colonel H. B. Fawcus and Mr. F. N. Doubleday.

Current Literature.

The Experimental Basis for a Specific Sero-diagnosis of Active Tuberculosis. By A. v. Wassermann (*Deutsche medizinische Wochenschrift*, March 9, 1923).—For a sero-diagnosis of tuberculosis we have in Koch's tuberculin an antigen which in delicacy can hardly be surpassed, but owing to this very sensitiveness it will show the actual presence of tubercle bacilli rather than indicate active tuberculosis, and it is this latter condition which it is necessary for the physician to diagnose. In the sero-diagnosis of syphilis it had been shown that a chemical antigen, lipoid in nature, can be employed in place of an antigen specific to the disease, and in this the reaction is held to be an indication of the tissue involvement rather than of the presence of the infective organism. In the tuberculous antigens of Besredka, Negre, and Bouqet, we have antigens which are far superior as such to the tubercle bacilli cultures previously used, but they have the disadvantage that they often react positively with the blood of syphilitics who do not show any signs of tuberculosis. The aim of W. is to obtain an antigen which will react to the tissue condition of tuberculosis but will be sufficiently specific to discriminate between this condition and that due to other infections. The accomplishment of this can only be established empirically by the results of clinical observations extending over years, but W. considers that the principles of his method for preparing antigen are on lines which should give the desired results.

The conclusions which he sets out at the termination of his paper are as follows :—

(1) The serum of tuberculous persons is of a very strongly-marked lipophil character.

(2) It, therefore, requires a definite quantity of phosphatids in the antigen and this characteristic places the serum in the same group sero-diagnostically as syphilis.

(3) The serum of tuberculous patients is distinguishable from that of syphilitics in that it does not react with lipoids alone, but requires as well another component in the antigen and this component is best contained in tubercle bacilli protoplasma.

(4) The serum of a tuberculous patient can, on the other hand, be distinguished from that of a healthy organism which has been treated with tubercle bacilli in that the former requires more lipoids in the antigen in order to produce a positive sero-diagnostic reaction.

(5) Preliminary fattening of the tubercle bacilli and subsequent addition of phosphatids furnish a possibility of providing an antigen which, in sero-diagnosis applied to man, only gives a specific sero-reaction with the serum of individuals who possess tuberculous tissue.

(6) This positive serological result is thus conclusive in diagnosing active tuberculosis.

The Prevention of Pneumonia by Pneumococcus Vaccine. By R. L. Cecil (*American Journal of Public Health*, xii, No. 3, March, 1923, p. 182.)—The author quotes experiences obtained during the war among troops as well as in civil practice and also experimental results which tend to show that a high degree of immunity, albeit somewhat transient, can be obtained against pneumonia by the administration of three injections of pneumococcus vaccine. He considers that the quantities usually employed are too small and advocates the use of a vaccine containing in 1 c.c. three billion of each of types I, II and III. The vaccine is administered subcutaneously and the reaction obtained is usually quite mild. Experimental evidence showed that monkeys could be readily immunized against pneumonia by three intratracheal injections of vaccine and experiments are in progress by which the author hopes that it will be possible to produce a non-toxic water-soluble extract of pneumococcus antigen which can be used for intratracheal immunization by means of an ordinary atomizer.

Résumé of the "Investigations into the occurrence and classification of the hæmoglobinophilic bacteria." Martin Kristensen (Chief of the Department of the State Serum Institute, Copenhagen). Copenhagen: Levin and Munksgaard, 1922, 272 pp., 3 plates.

(1) The occurrence of Pfeiffer's bacillus in influenza patients was investigated during four epidemics in the years 1918 to 1922. It was found only rarely in the first epidemic, while in the next, which was only separated from the first by an extremely short interval, it was found decidedly more frequently (in the sputum in thirty-five per cent of the patients and in fifty-one per cent of the autopsies on influenzal pneumonia). A more efficient technique would certainly have yielded a somewhat higher percentage in the case of the sputum.

Pfeiffer's bacillus must certainly have increased in distribution in influenza patients between the first and second epidemics. Whether any real change in its occurrence in influenza took place later cannot be decided.

(2) Pfeiffer's bacillus was found in influenza most often in the first couple of days after the beginning of the disease.

(3) In whooping cough this organism could be demonstrated still more frequently than in influenza, in the first series in all the twelve patients examined.

(4) In measles it had a similar distribution to that of influenza.

(5) It was found in pure culture in the spinal fluid of five cases of meningitis.

(6) Pfeiffer's bacillus was met with, widely distributed among healthy persons. The occurrence, however, was very variable in the different groups investigated ranging from eight per cent to 100 per cent.

(7) As a direct consequence of each of the four epidemics a marked increase in the distribution of Pfeiffer's bacillus among the population as

a whole was observed, followed in the succeeding months by a sharp decrease.

(8) In military camps and barracks Pfeiffer's bacillus was encountered in distinctly wider distribution than was usual in the remainder of the population.

(9) Pfeiffer's bacillus was found a little more commonly in persons who had had influenza than in others, but it may also occur in wide distribution among the latter.

From Nos. 1, 7 and 9 it is concluded that its distribution in influenza was secondary to the influenza and that its distribution in healthy subjects was probably secondary to its occurrence in influenza.

(10) In inoculations from the mouths of sixty guinea-pigs Pfeiffer's bacillus was found in five. It could not, however, be demonstrated in horses or mice.

(11) From the different sources named about 800 strains were cultivated. In all of these (with unimportant exceptions) the characters detailed below under *a*, *d*, *g*, *h*, and either *e* or *f*, were demonstrated. The other characters were identified in a smaller number, in most cases in about 150 strains. As these may practically be looked upon as random samples of the entire material it is justifiable to assume that the characters which were demonstrated only in these strains were common to the whole material, and to the complete flora of Pfeiffer's bacilli present in Denmark.

(*a*) Microscopically they were all rods which could however assume very different forms. The collection of forms for the whole group is very large but not unlimited. The most important common feature is the slenderness of the bacilli, at least in a number of the individuals in a culture.

The collection of forms of an individual strain often constitutes a considerable proportion of the forms of the whole group.

(*b*) They are non-motile.

(*c*) Spores can never be demonstrated.

(*d*) They are distinctly Gram-negative.

(*e*) In pure culture they cannot grow on ordinary peptone broth agar.

(*f*) In pure culture they are usually unable to grow on blood-free ascitic agar. Very occasionally, however, a slight growth is observed.

(*g*) For a rich growth red blood-cells or certain of their derivates are required in the medium. All the stains appear to react in the same manner towards the form of "hæmoglobin" supplied to them. Thus they grow poorly on blood agar with intact blood corpuscles and abundantly on agar with heated or pepsin-digested blood. Different preparations of hæmolysed blood can give growth of very dissimilar intensity. But all the strains go together, so that when for example, any one strain gave a particularly good growth (for that strain) on a certain medium, it was the same with all the other strains inoculated simultaneously.

The lowest hæmoglobin concentration which would support growth was about the same for all the strains examined.

It is, therefore, not only the dependence on "hæmoglobin," but also the special nature of that dependence which is common to the group.

This character persists practically unaltered even after a long period of cultivation.

(*h*) On suitable nutritive media growth proceeds rapidly so that isolated colonies are always visible after twenty-four hours' incubation at thirty-seven degrees.

(*i*) All the strains (at any rate with the technique used) proved to be decidedly aerobic.

(*j*) No difference could be shown to exist with regard to the optimum hydrogen ion concentration of the medium for growth.

(*k*) The minimum temperature at which growth took place lay between twenty degrees and twenty-five degrees.

(*l*) The cultures were killed by drying for twenty-four hours (under further detailed experimental conditions).

(*m*) Surface cultures were killed by remaining five weeks in the cold room without being subcultured.

(*n*) On blood agar (even with the addition of pepsin-digested blood which increased the growth considerably) no hæmolysis took place during a week's incubation.

(*o*) No appreciable fermentation power was observed in the liquid medium containing glucose employed.

(*p*) Arbutin was not broken down.

(*q*) No proteolytic action (for gelatine or coagulated serum) was found.

All these characters were common to all the strains examined.

The following characters were common to the large majority of strains.

(*r*) Increase of growth around colonies of other species of bacteria. In a few strains this character was absent or very poorly developed.

(*s*) Death after half an hour's heating at about fifty degrees. A few strains however, resisted a slightly higher temperature.

Since all the strains cultivated, so far as was investigated, possess such a large number of common characters, they ought, as has hitherto been the case, to be classed as the same species. This receives additional support from the following marked differences between these bacilli and closely related species.

(12) The hæmolytic hæmoglobinophilic bacilli differ from Pfeiffer's bacilli: (*a*) in the presence of hæmolysis; (*b*) in only occurring as saprophytes; (*c*) in a less strict dependence on hæmoglobin; and usually also (*d*) in a coarser morphology; and (*e*) in a slighter resistance to keeping.

(13) *Bacillus hæmoglobinophilus canis* differs from Pfeiffer's bacillus: (*a*) in its richer growth on blood agar; (*b*) in its growth not being increased by other bacteria; (*c*) in promoting the growth of Pfeiffer's bacillus; (*d*) in its greater resistance to drying; (*e*) in having well developed fermentative powers.

Otherwise these two species resemble Pfeiffer's bacillus in so many points that they are naturally regarded as closely related forms.

(14) Bordet's whooping-cough bacillus is considered to be more divergent. The aim here has been to show how constant and easily demonstrable the series characters are which distinguish it from Pfeiffer's bacillus. These characters are:—

(*a*) The appearance of the culture on blood agar.

(*b*) Hæmolysis of a definite kind on this medium.

(*c*) The slow growth.

(*d*) Growth on blood-free ascitic agar.

(*e*) No growth or slight growth on the media which are particularly suitable for Pfeiffer's bacillus.

(*f*) The growth is not promoted by other bacteria.

(*g*) It has itself a growth-promoting action on Pfeiffer's bacillus.

(*h*) The greater resistance against keeping.

(*i*) Specific agglutination and complement fixation.

Most of these points were investigated with seventy-five strains.

(15) Apart from a certain change in the intensity of growth on long-continued cultivation all the strains of this organism investigated entirely agreed with one another for all practical purposes on each of the points enumerated, and also in the absence of growth on ordinary agar in which they resemble Pfeiffer's bacillus.

(16) In contrast to this great homogeneity in the species Bordet's bacillus (*B. tussis convulsivæ*), there exist in the species Pfeiffer's bacillus (*B. influenzæ*)—in spite of a large number of common characters—a multiplicity of individual differences between the strains.

The most important of these are:—

(*a*) The division into indol-producers and non-indol-producers.

(*b*) The macroscopic appearance and consistence of the culture. This applies to mass cultures on different media and for single colonies.

(*c*) Agglutination and complement fixation.

(*d*) Minimum temperature for growth.

(16) The division into indol-producers and non-indol-producers is very sharp. It has, with perhaps a few exceptions, proved to be independent of long-continued cultivation and wide differences in the experimental conditions.

Strong and weak indol-producers can be distinguished. The latter constitute only a small minority.

On suitable media the strong indol-producers form about as much indol as *B. coli.*

(17) On the basis of the macroscopic appearance and consistence of the culture a classification of Pfeiffer's bacilli into two not very sharply defined main groups, "typical" and "atypical," is made. This difference is particularly well brought out by the same experimental arrangement as is employed to demonstrate the "symbiosis" phenomenon.

(18) A definite method for showing the symbiosis phenomenon has been evolved, based on the investigation of a series of different factors of which the following may be specially alluded to (19 to 21):—

(19) The medium must contain a small amount of "hæmoglobin"; details are given of the form in which it must be present.

(20) The cultures of the growth-promoting organism and Pfeiffer's bacillus must not be brought into immediate contact.

(21) It can be assumed that the different species of bacteria form the same growth-promoting substance, but in different amounts.

(22) There are strains which must be classed as Pfeiffer's bacilli although the symbiosis phenomenon is poorly developed or even completely absent.

(23) From investigations into the agglutination reactions it may be asserted that any two strains of Pfeiffer's bacillus chosen at random will prove in the majority of cases to be fundamentally distinct when compared by simple agglutination and absorption of agglutinins.

Out of six strains of Pfeiffer's bacillus cultivated from autopsies on influenzal pneumonia cases in the same epidemic, only two were identical as regards agglutination. The others were distinctly different from them and from one another.

(24) No connexion could be shown to exist between the agglutination reaction and the classification into indol-positive and indol-negative strains.

(25) The larger number of typical strains, however, are indol-producers, and the majority of atypical strains, non-indol-producers.

(26) As regards microscopic morphology there are in all probability true individual differences between strains of Pfeiffer's bacillus, but on account of the great variability of the individual strains and the gradual transitions between the different appearances observed it has been impossible to determine their exact nature.

The occurrence of round bodies has been shown to be a feature common to all Pfeiffer's bacilli, which exhibit wide variations with regard to size and staining reactions.

Some of these bodies are stained black by Gram's method and also with fuchsin or any simple staining method.

(27) A comprehensive survey is given of the way in which the hæmoglobinophilic bacteria probably ought to be classified. Only three species are considered as belonging to this group, namely, Pfeiffer's bacillus, the hæmolytic hæmoglobinophilic bacteria, and *B. hæmoglobinophilus canis.*

Of these the first species only is to be regarded as hæmoglobinophilic in the strictest sense.

Besides these three well-defined species a few strains have been described which may possibly belong to same group.

(28) An account and criticism of the nutritive media which have been used in the cultivation of Pfeiffer's bacillus have been given.

(29) From my own experience and that of others it is maintained that in all probability Pfeiffer's bacillus is not to be looked upon as the primary specific virus of influenza. A number of considerations of the conditions relating to its occurrence in the healthy and the sick are also given.

Our knowledge of the biology of the hæmoglobinophilic bacteria still requires to be widened in many directions.

[This valuable paper should be perused *in extenso* by those interested in the study of influenza.—EDITOR.]

Reviews.

THE REFORMATION OF WAR. By Colonel J. F. C. Fuller. $9\frac{1}{4} \times 6\frac{1}{4}$, xvi + 287 pp. Hutchinson and Co. Price 16s. net.

George III, in his day, said that Burke's book on the French Revolution should be in the hands of every gentleman; we, in our generation, say for analogous reasons that this book should be read by every citizen. Probably, not every reader will accept all the author's views and conclusions, but he will be compelled to think. The theme is war, its origin, purpose and practice; all these the author discusses with refreshing candour and common sense. The secret of the pathology of war lies in man's periodic reversion to primitive instinct; he is freed for a space from the fear of death; and for the nation or herd there is glory in death, in self-sacrifice and renunciation, as there was once glory for man in risking his life in the winning of his mate and in the protection of his family and lair. It follows from this that the origin and purpose of war is the desire of one nation to impose its will upon another. Although this is the objective of war, yet history shows that the purpose of an army or navy, in peace time, has little to do with war, its object being not freely to evolve but rather to mark time and maintain its traditions.

The dominant note in Colonel Fuller's book is to slay tradition and, believing that our Empire is founded on the vice of contempt, he endeavours to picture for us what the next war will be like, so as to stir us up to prepare for it. Since in war, as in all other phases of human activity, we find that the elemental psychological power is mind, with its desire to live as well as to kill, so the author argues that the most humane method of waging war in the future will be the moral attack on the enemy's nerves, and that the more horrible and terrifying the apparatus of war is made the sooner will a war be over, and the less will be the total injury to life and property. Further, we need to remember that in these days entire nations go to war, not only as soldiers but as the moral and material suppliers of soldiers. This being so, we cannot differentiate between the military and ethical objectives of nations at war; consequently, to attack the civilian workers of a nation is as justifiable an act of war as to attack its soldiers, especially as the solidarity of its fighting forces is founded on the civil will. The tactical tendency of future warfare will be, therefore, to strike at the moral rather than at the muscle of an enemy, and, according to our author,

the essential agents will be tanks, aeroplanes and gas. The first two being vehicles and the third the economical weapon.

As a late general-staff officer of the Tank Corps, Colonel Fuller is enthusiastic regarding the future of the tank. We do not profess to be competent to criticize his views as to this arm, but we do know some soldiers who regard the tank as a freak called into existence by exceptional circumstances which are not likely to recur. Whether this be so or not, there can be no doubt that the real place for tanks is in front and not behind infantry. As regards aeroplanes and gas, he is on surer ground, subject to the conditions that the aeroplane be regarded rather as a means of offence than of defence. Gas he rightly regards as the weapon of the future because chemical warfare has come to stay, and the lesson is obvious that no nation can hope to protect its fighting forces effectively against gas unless offensive gas warfare is studied during peace time and the troops themselves trained to understand what this form of warfare entails. We think the author is wrong, however, in stating that if a new gas is discovered an immediate antidote can be assured as forthcoming. If anyone is inclined to take refuge in this belief let him try to visualize what would happen to-morrow if hostile aircraft were to drop a few tons of mustard gas or some of the organic arsenic compounds on to an area defined by a two mile radius from St. Paul's in London. If to these were added some tons of toxic smokes or particulate clouds the fog of death would remain for days, and there would be neither time nor means for antidotes. We regard the chapter on gas as the weapon of the future as being quite the best in this book. It might well have been longer and more emphatic, because until we in this country have given full consideration to the question of chemical armament and chemical disarmament and their associated problems, of which perhaps air defence is the chief, we live in a fool's paradise and when the next war comes will stand condemned of the most criminal negligence.

The later chapters of this book are devoted to the future of air, land and sea warfare, followed by others on the somewhat polemical subjects of imperial defence, grand strategy and the reformation of the Army. They all afford entertaining and instructive reading. We are not prepared to criticize as they touch upon some questions as to which we are imperfectly informed. The author handles his subjects with ability and evidently has very clear and dogmatic views. So much so that it is to these chapters of constructive criticism he is likely to meet with the greatest opposition. Fortunately, he is now an instructor at Camberley where he will have every opportunity of inspiring the would-be staff officer with his views and enthusiasms for mechanicalizing the Army. Although we cannot go all the way with him, we confess assent to much of what he says. He writes with the rush-light of common sense and pleads that our future soldiers be not asked to advance under the faded banners of tradition and that they be trained or that their actions may be adapted to the circumstances which

will then confront them. He may or may not be a prophet crying in the wilderness but this much is certain: the question discussed is not only for the professional soldier, it is one which very much concerns the ordinary citizen. It is he that pays for the army which maintains the inviolability of his home and the peace which he enjoys. And, as the man who pays, he has the right to decide whether he is getting value for his money; but he must remember that if any change is needed it is only he who can effect the change, for the soldier is the instrument of civilian policy and not its fashioner. This book attempts to outline a policy as indicated by recent experience. R. H. FIRTH.

CHART TO TELL WHETHER AN INDIAN SNAKE IS POISONOUS OR HARMLESS. John Bale, Sons and Danielsson, Ltd., 83-91, Great Titchfield Street, Oxford Street, W. 1, or obtainable from the Bombay Natural History Society, 6, Apollo Street, Bombay.

This chart is published in two sizes, a small folding pocket edition, and on an enlarged scale thirty by forty inches in map form mounted on varnished linen and fixed between wooden rollers. In this excellent chart the essential features of distinction between a poisonous or a non-poisonous Indian snake are clearly depicted and briefly explained in a few words of letterpress sufficient to enable a medical officer to pronounce at once whether a snake submitted to him for identification is harmless or poisonous, and if the latter, to enable him to identify the family, Colubridæ or Viperidæ, to which the snake belongs.

The chart undoubtedly supplies a want, and the pocket edition should have a place in the library of every medical officer serving in India. The map form is suitable for hanging on the walls of hospitals, dispensaries, schools, etc.

Correspondence.

THE CAUSE OF INFLUENZA.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—It is a pleasure to note that in Colonel Gordon's letter in your April issue there is now a little less of the "filter-passer" and rather more of Pfeiffer's bacillus. That is all to the good, because the more the question of the biology of this most interesting organism is threshed out the sooner we shall arrive, in my opinion, at a complete comprehension of its *modus operandi* in relation to the causation of influenza.

However, in order not to appear to slight the "filter-passer," I will give it the place of honour in this reply. I wish I could emulate Colonel

Gordon's comfortable, assured style of writing. He continues calmly to maintain the organismal nature of this creation, but he does not vouchsafe any definite answer to my queries as to (*a*) the recognition or not of the occurrence of discrete coccoid particles of proteid nature resulting from the lysis of cellular elements, and (*b*) how the "organism" is to be distinguished from such particles. I know Colonel Gordon thinks I am always repeating myself, but these are points which *have* to be satisfactorily dealt with before the view that an independent organism is present can be accepted. If Colonel Gordon will do me the honour of reading my paper on the "Rickettsias," he will see that a precisely similar question has to be answered in their case also. How is a true *Rickettsia* to be distinguished? Failing such distinction, in neither case do I consider that there is any logical reason to postulate the existence of a new type of living organism. Now, in both cases I am not simply making *ex cathedra* statements, because I have unmistakable evidence of the mode of origin of such granular elements; but, to set against this, what real evidence is there of the organismal nature of any of them?

With all deference, I hardly think Colonel Gordon is correct in implying that Sir Spencer Lister's work is any confirmation of the matter under discussion. Surely Lister's work does not carry us any further as regards the *real nature* of the virus, or pathogenic agent (*admittedly* present), which is the point at issue.

Well, I must now leave the "filter-passer" to the care of its enthusiasts. Of course, I may be mistaken, but "I think—mind, I only say I think" (if I may quote from my favourite opera) that this important question will ultimately be decided in the negative. I have put forward my own view, or explanation, because I thought that it might—and I still believe it will —provide a necessary link in the chain of a complete understanding of the biological relation of the influenza bacillus to the tissue-cells, and at the same time account for the pathogenic effects of the bacillus-free "cultures." Time alone can show.

Colonel Gordon rightly says that more pathological evidence is required in order to solve the problem of the ætiology of the disease. By a happy coincidence, almost simultaneously with the appearance of his letter, there was published in the *British Medical Journal* and the *Lancet* of March 31 an account of a lecture on the pathology of influenza given by one of the greatest living pathologists. And Colonel Gordon has probably noted that two of Adami's conclusions were: (1) one agent and one only was primarily responsible for the cases of the pandemic; and (2) this agent was the influenza bacillus. Adami considers that one and the same organism might cause different lesions in different stages of the epidemic owing to its having acquired a higher degree of virulence as the epidemic progressed; and he gives as an illustration a classical observation made by Metchnikoff. In other words, Adami inclines to a view and interpretation of the activities of Pfeiffer's bacillus similar to that expressed by

Levinthal and Fernbach in the paper to which I referred. And I can find not the slightest reference to the "filter-passer"! One would like to know, therefore, how this considered pathological judgment of Adami appeals to Colonel Gordon.

Now, of course, on the bacteriological side, as between Levinthal and Fernbach and Kristensen, I am not, unfortunately, able to give any competent opinion. But if I may venture to offer a few remarks, it does seem to me that Colonel Gordon dismisses somewhat casually the former authors' view that there is a strain or variety of the influenza bacillus which is hæmolytic.

In the first place, certain of the protocols of the cases given by Levinthal and Fernbach, indicating the variation in type of the organism (including "bacillus X") at different periods, are, to my mind, suggestive of a specific connexion; and, it might almost be added, of a pathogenic relation of the hæmolytic type to certain bronchitic lesions. Indeed the described condition of the patients at the time of the occurrence, in certain cases, of the hæmolytic type, reads, at any rate, quite like that on which special emphasis was laid by Adami (*loc. cit.*).

Again, as regards one important biological character, the two accounts are at variance. Whereas none of the hæmolytic strains investigated by Kristensen lost the property of hæmolysis on continued cultivation, certain of those isolated by Levinthal and Fernbach did become ultimately non-hæmolytic. Needless to say, a single positive observation entirely upsets any amount of negative evidence. And this is, I think, a most interesting point. Moreover, it is corroborated by a similar observation of the loss of hæmolytic power made by Rivers and Leuschner (*Bull. Johns Hopkins Hosp.*, vol. xxxii, 1921, p. 130). As these authors point out, such a type is then an influenza bacillus.

Further, in addition to these workers, Bloomfield also considers that there are hæmolytic influenza bacilli. This author, indeed, says that such a hæmolytic type "is culturally and morphologically essentially identical with the influenza bacillus, except for its hæmolytic property"—which we have seen may be lost.

Even Kristensen himself agrees that in some cases the microscopic picture of this hæmolytic type is indistinguishable from that of microscopically atypical Pfeiffer's bacilli. The spherical bodies were often well developed but scarcely differed from the corresponding bodies in Pfeiffer's bacillus.

Altogether, therefore, I submit that Colonel Gordon scarcely accords to this hæmolytic type that appreciation of its importance in relation to the influenza bacillus and to influenzal pathology, which it may really deserve. My own opinion, for what it may be worth, is that Levinthal and Fernbach are just as likely to be right as is Kristensen; and that this hæmolytic type may be an atypical form or strain of Pfeiffer's bacillus just as much as the other "pseudo" types admittedly are.

I am afraid, Sir, this letter is becoming as usual, too long. But perhaps I may be allowed to add that, to my mind, the most surprising feature about Kristensen's undoubtedly useful account of the influenza bacillus is that nowhere does he offer the slightest suggestion as to what he would replace the deposed Pfeiffer's bacillus with, as the primary cause of influenza! Agreed, that the work is mainly a bacteriological study, nevertheless, the author does consider the general problem of the ætiology. And after he has endeavoured to pull down a substantial edifice which seems only to require a little extension and alteration, one would have welcomed at least an attempt to build something concrete in its place. What causes "influenza taken in its widest sense"? Further, influenza is by no means always only a sequel to measles, whooping cough, catarrh, etc. The strong healthy soldiers, stricken down in numbers like nine-pins, suffered from none of these things! I think you must have a specific infective agent, its activities varying *according to the biological state both of organism and host*. What does Kristensen consider this agent is? I must confess that Kristensen's view seems to me largely to "beg the question"; especially when Colonel Gordon's comment is that the prophylactic use of a vaccine containing Pfeiffer's bacillus finds substantial support from his work.

Kristensen circles round and round the Pfeiffer bacillus without really either accepting or rejecting it, although he is, admittedly, greatly impressed by it. The long and the short of the matter is: *one cannot get away from Pfeiffer's bacillus*. And a perusal of Kristensen's conception of its activities only leads one nearer to the conclusion that a type of enhanced virulence *is* the agent responsible for epidemics. Indeed, the author himself almost admits as much when he says that "perhaps the microbe eventually acquires so great an increase in its vitality that it thrives not only as a saprophyte in healthy persons, but also may be able by itself to produce infections resembling influenza"! What is this but a cautious essay in the direction of Levinthal and Fernbach's and also Adami's view? But Colonel Gordon apparently thought this sentence could be omitted as unimportant.

Lastly, if I may use a metaphor inevitably suggested by what we see everywhere around us to-day, What of certain vibrations which have been repeatedly emanating from the Rockefeller Institute during the last two years? Had they not arrived at the Copenhagen Institute, or could it possibly be that the receiver there was not sympathetically attuned to them?

I am, Sir, etc.,
H. M. WOODCOCK.

OFFICERS HOSPITAL, 4, PERCIVAL TERRACE, BRIGHTON.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

We have received for publication, the following letter from the Joint Council of the Order of St. John of Jerusalem in England and the British Red Cross Society.

19, *Berkeley Street,*
London, W. 1.
May 7, 1923.

DEAR SIR JOHN,—We have at the present time a number of vacancies in our hospital, at 4, Percival Terrace, Brighton, and it may not perhaps be very generally known that, besides taking patients suffering from Service disabilities, we are prepared to receive officers and ex-officers needing surgical, medical or convalescent treatment for complaints not directly attributable to the war.

We have found it necessary to ask any patients not paid for by a Government department to contribute 4s. 6d. per day towards their maintenance expenses, but should an applicant's financial circumstances preclude him from meeting this charge, we should always be prepared to consider the question of reducing it.

As you know, we are able to treat practically any type of case in our Brighton Hospital, and I should be very glad if you could make it known to as many officers as possible that these facilities exist, and that we are anxious that every advantage should be taken of them.

Yours sincerely,
GEORGINA DUDLEY.

Lieutenant-General Sir John Goodwin, K.C.B.,
War Office, Whitehall, S.W. 1.

INDEX TO VOLUME XL.

C.N. = Clinical and other Notes.
C.L. = Current Literature.

Notices.

EDITORIAL NOTICES.

The Editor will be glad to receive original communications upon professional subjects, travel, and personal experiences, etc. He will also be glad to receive items of news and information regarding matters of interest to the Corps from the various garrisons, districts, and commands at home and abroad.

A free issue of twenty-five reprints will be made to contributors of Original Communications and of twenty-five excerpts of Lectures, Travels and Proceedings of the United Services Medical Society.

Any demand for *reprints, additional to the above*, or for excerpts must be forwarded at the time of submission of the article for publication.

Matter intended for the Corps News should reach the Editor not later than the 15th of each month for the following month's issue. Notices of Births, Marriages, and Deaths are inserted free of charge to subscribers. All these communications should be written upon one side of the paper only; they should by preference be type-written; but, if not, all proper names should be written in capital letters (or printed) to avoid mistakes, and be addressed: The Editor, "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS," War Office, Whitehall, S.W.1.